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SOMMAIRE

Logical positivism [1]

by Bertrand RUSSELL

"Logical Positivism" is a name for a method, not for a certain kind of result. A philosopher is a logical positivist if he holds that there is no special way of knowing that is peculiar to philosophy, but that questions of fact can only be decided by the empirical methods of science, while questions that can be decided without appeal to experience are either mathematical or linguistic. Many members of the school would describe their position briefly as a determination to reject "methaphysics", but "metaphysics" is such a vague term that this description has no precise meaning. I should prefer to say that questions of fact cannot be decided without appeal to observation. For example: seventeenth century continental philosophers would contend that the soul must be immortal because the soul is a substance and subtances are indestructible. A logical positivist would reject this argument, but would not necessarily hold that the soul is mortal, since he might think that psychical research gives empirical evidence of survival.

This, however, does not distinguish the logical positivist from earlier empiricists. What is distinctive is attention to mathemathics and logic, and emphasis upon linguistic aspects of traditional philosophical problems. British empiricists, from Locke to John Stuart Mill, were very litle influenced by mathematics, and had even a certain hostility to the outlook engendered by mathematics. On the other hand, Continental philosophers, down to Kant, regarded

[1] Parts of this article are contained in my recent book, *Human Knowledge, its scope and limits.*

mathematics as the pattern to which other knowledge ought to approximate, and thought that pure mathematics, or a not dissimilar type of reasoning, could give knowledge as to the actual world. The logical positivists, though they are as much interested in and influenced by mathematics as Leibniz or Kant, are complete empiricists, and are enabled to combine mathematics with empiricism by a new interpretation of mathematical propositions. It was work on the foundations of mathematics and on mathematical logic that gave the technical basis for the school, and without some understanding of this basis it is impossible to do justice to the grounds for their opinions.

Mathematics, from Pythagoras onward, was mixed up with mysticism. Plato's eternal world was inspired by mathematics. Aristotle, though more empirical and more biological than Plato, still thought the capacity for doing sums so remarkable that the arithmetical part of the soul must be immortal. In modern times, Spinoza took geometry for his model, and hoped to deduce the nature of the universe from self-evident axioms, and Leibniz thought that all questions could be decided by reasoning, as appears in his account of what could be achieved by the Characteristica Universalis: "If we had it, we should be able to reason in metaphysics and morals in much the same way as in geometry and analysis," [1] and again: "If controversies were to arise, there would be no more need of disputation between two philosophers than between two accountants. For it would suffice to take their pencils in their hands, to sit down to their slates, and to say to earch other (with a friend as witness, if they liked): Let us calculate." [2]

Kant's theory of knowledge cannot be disentangled from his belief that mathematical propositions are both synthetic and *a priori*. My own work on the principles of mathematics was to me, at first, mainly interesting as a refutation of the view that mathematical propositions assert more than can be justified by deductive logic.

Hegel (especially in his *Greater Logic*) made a quite different use of mathematics. The great men of the seventeenth

[1] Gerhadt's edition, Vol. VII, p. 21.
[2] *Ibid.*, p. 200.

and eighteenth centuries were so much impressed by the *results* of their new methods that they did not trouble to examine their foundations. Although their arguments were fallacious, a special Providence saw to it that their conclusions were more or less true. Hegel fastened upon the obscurities in the foundations of mathematics, turned them into dialectical contradictions, and resolved them by nonsensical syntheses. It is interesting that some of his worst absurdities in this field were repeated by Engels in the *Anti-Dühring*, and that, in consequence, if you live in the Soviet Union and take account of what has been done on the principles of mathematics during the last 100 years, you run a grave risk of being liquidated.

Let us enumerate a few of the errors that infected mathematics in the time of Hegel. There was no defensible definition of irrational numbers, and consequently no ground for the Cartesian assumption that the position of any point in space could be defined by three numerical coordinates. There was no definition of continuity, and no known method of dealing with the paradoxes of infinite number. The accepted proofs of fundamental propositions in the differential and integral calculus were all fallacious, and were supposed, not only by Leibniz, but by many later mathematicians, to demand the admission of actual infinitesimals. As regards geometry, it was supposed that the truth of the Euclidean system could be known without any appeal to observation.

The resulting puzzles were all cleared up during the nineteenth century, not by heroic philosophical doctrines such as that of Kant or that of Hegel, but by patient attention to detail. The first step was Lobatchevsky's non-Euclidean geometry, which showed that only empirical observation can decide whether Euclidean geometry is true of actual space, and that geometry as a part of *pure* mathematics throws no more light upon actual space than the multiplication table throws on the population of an actual town.

The next step was Weierstrass's work on the differential and integral calculus, which eliminated infinitesimals and substituted limits. Then came Georg Cantor's definition of continuity and arithmetic of infinite numbers. And last came Frege's definition of cardinal numbers, and his proof that

arithmetic requires no concepts and no premises that are not required in deductive logic. It seems strange that, although numbers had been used for many thousands of years, no one could define either "number" or any particular number until Frege did so in 1884. And what is perhaps even more strange is that no one noticed what Frege had achieved until I read him 18 years later.

The definition of the number 1 had great importance in clearing up metaphysical confusions. "One" is a predicate of certain classes, e. g., "satellite of the earth"; but when a class has only one member, it is nonsense (in the strict sense) to say that that member is one, unless the unit class was a class of classes, in which case it is generally false. E. g. you may say: "In such-and-such a Parliament there is only one political party", but the party is not one, unless it has only one member. More generally, if I say "there are three men in the room," the correct statement is "the class of men in the room is a triad". This may seem a trivial matter, but it is amazing how much bad metaphysics it refutes.

For example: the scholastics used to say "*One* and *Being* are convertible terms". It now appears that "one" is a predicate of concepts, not of the things to which the concepts are applicable; the predicate "one" applies to "satellite of the earth" but not to the moon. And for other reasons "being" applies only to certain descriptions, never to what they describe. These distinctions, it will be found, put an end to many arguments of metaphysicians from Parmenides and Plato to the present day.

This development in the principles of mathematics suggested that philosophical puzzles are to be solved by patience and clear thinking, the result being, in very many cases, that the original question is shown to be nonsensical. Logical positivism arose largely out of this suggestion. Carnap maintained at one time that *all* philosophical problems arise from errors in syntax, and that, when these errors are corrected, the problems either disappear or are obviously not soluble by argument. I do not think he would still maintain quite so extreme a position, but there can be no doubt that correct logical syntax has an importance which was not for-

mely recognised, and which logical positivists have rightly emphasised.

Wittgenstein's *Tractatus Logico-Philosophicus*, published shortly after the first world war, laid great emphasis upon syntax, and provided a stimulus which helped in the formation of the "Wiener Kreis", where first logical positivism took the form of a definite school. The Wiener Kreis and the admirable periodical *Erkenntnis* did excellent work, until they were ended by Hitler and the *Anschluss*. It was by the Vienna school that the hierarchy of languages was developed, a doctrine which I had briefly suggested as a way of escaping from Wittgenstein's rather peculiar syntactical mysticism. He had maintained that the *form* of a sentence can only be *shown*, not *stated*. The apprehension of *form*, in his doctrine, was something that was ineffable in the strict sense, and only possible in virtue of some kind of mystical insight. This point of view was very alien to the spirit of logical positivism. It was therefore natural that the Wiener Kreis, admitting Wittgenstein's problem, should seek other ways of solving it.

It appeared that, given any language, it must have a certain incompleteness, in the sense that there are things to be said *about* the language which cannot be said *in* the language. This is connected with the paradoxes—the liar, the class of classes that are not members of themselves, etc. These paradoxes had appeared to me to demand a hierarchy of "logical types" for their solution, and the doctrine of a hierarchy of languages belongs to the same order of ideas. For example, if I say "all sentences in the language L are either true or false", this is not itself a sentence in the language L. It is possible, as Carnap has shown, to construct a language in which many things about the language can be said, but never *all* the things that might be said: some of them will always belong to the "metalanguage". For example, there is mathematics, but however "mathematics" may be defined, there will be statements *about* mathematics which will belong to "metamathematics", and must be excluded from mathematics on pain of contradiction.

There has been a vast technical development of logic, logical syntax, and semantics. In this subject, Carnap has done the most work. Tarski's *Der Begriff der Wahrheit in*

den formalisierten Sprachen is a very important book, and if compared with the attempts of philosophers in the past to define "truth" it shows the increase of power derived from a wholly modern technique. Not that difficulties are at an end. A new set of puzzles has resulted from the work of Gödel, especially his article *Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme* (1931), in which he proved that in any formal system it is possible to construct sentences of which the truth or falsehood cannot be decided within the system. Here again we are faced with the essential necessity of a hierarchy, extending upwards *ad infinitum*, and logically incapable of completion.

This whole subject has become so technical, and so capable of quasi-mathematical definiteness, that it can hardly be regarded as belonging to philosophy as formely understood. True, it solves what *were* philosophical problems, but so did Newton in writing on what he still called "natural philosophy". But we do not now regard planetary theory as part of philosophy, and I think that on the same ground much of the recent work on logic, syntax, and semantics should be regarded as definite knowledge, not philosophical speculation.

So far, I have been dealing with topics that arise out of the consideration of mathematics and logic. I come now to what logical positivism has to say about empirical knowledge, and here I find myself, on some important points, no longer in agreement with most members of the school.

There are here two not wholly disconnected questions. The one is as to scientific as opposed to deductive inference; the other is as to what is meant by the "significance" of a sentence.

The question of scientific inference is one which has been acute ever since the time of Hume. It has been common to include under "induction" all such inferences as would be considered valid in science but are not justified by the rules of deduction. I think myself that induction has less importance in this problem than is generally thought. What is clear, and generally admitted, is: (1) That scientific as opposed to deductive inference can only make the conclusion probable; (2) That it cannot even do this except by assuming postulates, or a postulate, for which there is, and can be, no

empirical evidence. This is an awkward conclusion for an empiricist, but it seems to be unescapable. I shall not in this article deal further with this problem, but shall instead examine the doctrine that "significance" and "knowledge" are both confined to experience.

Some modern empiricists—in particular, the majority of logical positivists—have, in my opinion, misconceived the relation of knowledge to experience. This has arisen, if I am not mistaken, from two errors: first, an inedequate analysis of the concept "experience", and second, a mistake as to what is involved in the belief that some assigned property belongs to some (undetermined) subject. Two specific problems arise, one as regards significance, the other as regards knowledge of what are called "existence propositions", i. e. propositions of the form "something has this property". It is maintained, on the one hand, that a statement is not "significant" unless there is some known method of verifying it; on the other hand, that we cannot know "something has this property" unless we can mention a specific subject that has the property. I wish to give reasons for rejecting both these opinions.

Before examining the abstract logic of these two problems, let us consider them, for a moment, from a common-sense point of view.

To begin with verification: There are some who maintain that, if atomic warfare is not checked, it may lead to the extermination of life on this planet. I am not concerned to maintain that this opinion is true, but only that it is significant. It is, however, one which cannot be verified, for who would be left to verify it if life were extinct? Only Berkeley's God, whom, I am sure, logical positivists would not wish to invoke. Going backwards instead of forwards, we all believe that there was a time before there was life on the earth. Those who regard verifiability as necessary to significance do not mean to deny such possibilities, but in order to admit them they are compelled to define "verifiability" somewhat loosely. Sometimes a proposition is regarded as "verifiable" if there is any empirical evidence in its favour. That is to say, "all A is B" is "verifiable" if we know of one A that is B and do not know of one that is not B.

This view, however, leads to logical absurdities. Suppose there is no single member of A concerning which we know whether it is a B, but there is an object x, not a member of A, which we know to be a B. Let A' be the class consisting of the class A together with the object x. Then "all A' is B" is verifiable in terms of the definition. Since this implies "all A is B", it follows that "all A is B" is verifiable if there is, anywhere, a single object known to be a B.

Consider now a generalization of a different sort, such as we may wish to make in connection with the doctrine of natural kinds. The generalizations I have in mind are those of the form: "all predicates of the class A are true of the object B".

Applying the same definition of "verifiability", this is "verifiable" if some, or at least one, of the predicates of the class A is empirically known to be true of B. If this is not the case, let P be some predicate known to be true of B, and let A' be the class consisting of the class A together with P. Then "all predicates of the class A' are true of B" is verifiable, and so, therefore, is "all predicates of the class A are true of B".

From these two processes it follows that, if anything is known to have any predicate, al generalizations are "verifiable". This consequence was not intended, and shows that the above wide definition of "verifiability" is useless. But unless we allow some such wide definition, we cannot escape from paradoxes.

Let us next consider propositions containing the word "some", or an equivalent, e. g. "some men are black", or "some quadrupeds have no tails". As a rule, such propositions are known by means of instances. If I am asked "how do you know that some quadrupeds have no tails?" I may reply "because I once had a Manx cat, and it had no tail". The view which I wish to combat maintains that this is the only way of knowing such propositions. This view has been maintained by Brouwer in mathematics, and is maintained by some other philosophers in regard to empirical objects.

The paradoxes resulting from this opinion are very similar to those resulting from the above doctrine as to verifiability.

Take such a proposition as "rain sometimes falls in places where there is no one to see it". No sane person would deny this, but it is impossible to mention a raindrop that has never been noticed. To deny that we know that there are occurences not observed by any one is incompatible with common sense, but is necessary if we never know such propositions as "there are A's" except when we can mention A's that we have observed. Can any one seriously maintain that the planet Neptune or the Antartic Continent did not exist until it was discovered? Again only a Berkelian God will enable us to escape from paradoxes. Or again: we all believe that there is iron in the interior of the earth, but we cannot give instances beyond the depth of the deepest mine.

Adherents of the doctrine that I am combating interpret such facts hypothetically. They say that the statement "there is undiscovered iron" is an abbreviation, and that the full statement should be: "if I did certain things, I should discover iron". Suppose, for the sake of precision, we take the statement "there is iron more than 1.000 kilometers below the surface of the earth". It is unlikely that anybody will ever find this iron, and, in any case, how can it be known what a person would find? Only by knowing what is there to be found. A hypothetical of which the hypothesis will probably always be false tells us nothing. Or consider: "there was once a world without life". This cannot mean: "If I had been alive then, I should have seen that nothing was alive".

Let us now consider the above two doctrines more formally, from a strictly logical point of view.

A. *Meaning and Verification*

There is a theory that the meaning of a proposition consists in its method of verification. It follows (*a*) that what cannot be verified or falsified is meaningless, (*b*) that two propositions verified by the same occurrences have the same meaning.

I reject both, and I do not think that those who advocate them have fully realized their implications.

First: practically all the advocates of the above view regard verification as a *social* matter. This means that they

take up the problem at a late stage, and are unaware of its earlier stages. Other people's observations are not data for me. The hypothesis that nothing exists except what I perceive and remember is for me identical, in all its verifiable consequences, with the hypothesis that there are other people who also perceive and remember. If we are to believe in the existence of these other people—as we must do if we are to admit testimony—we must reject the identification of meaning with verification.

"Verification" is often defined very loosely. The only strict meaning of verification is the following: A proposition asserting a finite number of future occurrences is "verified" when all these occurrences have taken place, and are, at some moment, perceived or remembered by some one person. But this is not the sense in which the word is usually employed. It is customary to say that a general proposition is "verified" when all those of its consequences which it has been possible to test have been found to be true. It is always assumed that, in that case, probably the consequences which have not been tested are also true. But this is not the point with which I am concerned at present. The point with which I am concerned at the moment is the theory that two propositions whose verified consequences are identical have the same significance. I say "verified", not "verifiable"; for we cannot know, until the last man perishes, whether the "verifiable" consequences are identical. Take, e. g. "all men are mortal". It may be that on February 9th, 1991, an immortal man will be born. The presently verifiable consequences of "all men are mortal" are the same as those of "all men born before the time *t* are mortal, but not all those born later", where *t* is any time not more than a century before the present.

If we insist upon using the word "verifiable" rather than "verified", we cannot know that a proposition is verifiable, since this would involve knowledge of an indefinitely long future. In fact, that a proposition is verifiable is itself not verifiable. This is because to state that all the future consequences of a general proposition are true is itself a general proposition of which the instances cannot be enumerated, and no general proposition can be established on purely empirical

evidence except one applying to a list of particulars all of which have been observed. E. g. I may say "the inhabitants of such-and-such a village are Mr. and Mrs. A, Mr. and Mrs. B, etc., and their families, all of whom are known to me personnally; and all of them are Welsh".[1] But when I cannot enumerate the members of a class, I cannot, on purely empirical grounds, justify any generalization about its members except what follows analytically from its definition.

There is however still a point to be made in favour of the verifiers. They contend that there is a distinction between two kinds of cases. In one, we have two propositions whose consequences hitherto have been indistinguishable, but whose future consequences may diverge; e. g. "all men are mortal" and "all men born before A. D. 2000 are mortal". In the other, we have two propositions whose observable consequences can never diverge; this is especially the case with metaphysical hypotheses. The hypothesis that the starry heavens exist at all times, and the hypothesis that they only exist when I see them, are exactly identical in all those of their consequences that I can test. It is specially in such cases that meaning is identified with verification, and that, therefore, the two hypotheses are said to have the same significance. And it is this that I am specially concerned to deny.

Perhaps the most obvious case is other people's minds. The hypothesis that there are other people, having thoughts and feelings more or less like my own, does not have the same significance as the hypothesis that other people are only parts of my dreams, and yet the verifiable consequences of the two hypotheses are identical. We all feel love and hate, sympathy and antipathy, admiration and contempt, for what we believe to be real people. The *emotional* consequences of this belief are very different from those of solipsism, though the *verifiable* consequences are not. I should say that two beliefs whose emotional consequences differ have substantially distinct significations.

But this is a practical argument. I should go further, and say, as a matter of pure theory, that you cannot, without

[1] Such general enumerative statements involve many difficulties, but I will ignore them.

incurring an endless regress, seek the significance of a proposition in its consequences, which must be other propositions. We cannot explain what is the significance of a belief, or what makes it true or false, without bringing in the concept "fact", and when this is brought in the part played by verification is seen to be subsidiary and derivative.

B. *Inferential Existence-Propositions*

A form of words containing an undetermined variable —for instance, "x is a man"— is called a "propositional function" if, when a value is assigned to the variable, the form of words becomes a proposition. Thus "x is a man" is neither true nor false, but if for "x" I put "Mr. Jones" I get a true proposition, and if I put "Mrs. Jones" I get a false one.

Besides giving a value to "x", there are two other ways of obtaining a proposition from propositional function. One is to say that the propositions obtained by giving values to "x" are all true; the other is to say that at least one of them is true. If "$f(x)$" is the function in question, we will call the first of these "$f(x)$ always" and the second "$f(x)$ sometimes" (where it is understood that "sometimes" means "at least once"). If "$f(x)$" is "x is not a man or x is mortal", we can assert "$f(x)$ always"; if "$f(x)$ is "x is a man", we can assert "$f(x)$ sometimes", which is what we should commonly express by saying "there are men". If "$f(x)$" is "I met x and x is a man", "$f(x)$ sometimes" is "I met at least one man".

We call "$f(x)$ sometimes" an "existence-proposition" because it says that something having the property $f(x)$ "exists". For instance, if you wanted to say "unicorns exist", you would first have to define "x is a unicorn" and then asert that there are values of x for which this is true. In ordinary language, the words "some", "a", and "the" (in the singular) indicate existence-propositions.

There is one obvious way in which we get to know existence-propositions, and that is by means of instances. If I know "$f(a)$", where a is some known object, I can infer "$f(x)$ sometimes". The question I wish to discuss is whether this is the *only* way in which such propositions can come to be known. I wish to maintain that it is not.

In deductive logic, there are only two ways in which existence-propositions can be proved. One is the above, when "$f(x)$ sometimes" is deduced from "$f(a)$"; the other is when one existence proposition is deduced from another, for instance "there are bipeds" from "there are featherless bipeds". What other methods are possible in non-deductive inference?

Induction, when valid, gives another method. Suppose there are two classes A and B and a relation R, such that, in a number of observed instances, we have (writing "a R b" for "a has the relation R to b")

a_1 is an A. b_1 is a B. a_1 R b_1
a_2 is an A. b_2 is a B. a_2 R b_2

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a_n is an A. b_n is a B. a_n R b_n

and suppose we have no contrary instances. Then in all observed instances, if a is an A, there is a B to which a has the relation R. If the case is one to which induction applies, we infer that probably every member of A has the relation R to some member of B. Consequently, if a_{n+1} is the next observed member of A, we infer as probable: "there is a member of B to which a_{n+1} has the relation R. We infer this, in fact, in many cases in which we cannot adduce any particular member of B such as we have inferred. We all believe that probably Napoleon III had a father, although no one has ever known who he was. Not even a solipsist, if he allows himself any views as to his own future, can escape from this sort of induction. Suppose, for instance, that our solipsist suffers from intermittent sciatica, which comes on every evening; he may say, on inductive grouds, "probably I shall be suffering pain at 9 p. m. tonight". This is an inference to the existence of something transcending his present experience. "But", you may say, "it does not transcend his *future* experience". If the inference is valid it does not; but the question is: "how is he to know *now* that the inference is probably valid?". The whole practical utility of scientific inference consists in giving grounds for anticipating the future; when the future has come and has verified the inference, memory has replaced inference, which is no longer needed. We must, therefore, find grounds for trusting the

inference *before* it is verified. And I defy the world to find any such grounds for trusting inferences which will be verified, which are not equally grounds for trusting certain inferences which will be neither verified nor falsified, such as the inference to Napoleon III's father.

We are faced with the question: in what circumstances is induction valid? It is futile to say: "Induction is valid when it infers something which subsequent experience will verify". This is futile, because it would confine induction to cases in which it is useless. We must have reasons, in advance of experience, for expecting something, and exactly similar reasons may lead us to believe in something that we cannot experience, for example, the thoughts and feelings of other people. The plain fact is that much too much fuss is made about "experience".

Experience is needed for ostensive definition, and therefore for all understanding of the meanings of words. But the proposition "Mr. A had a father" is completely intelligible even if I have no idea who Mr. A's father was. If Mr. B was in fact Mr. A's father, "Mr. B" is not a constituent of the statement "Mr. A had a father", or, indeed of any statement containing the words "Mr. A's father" but not containing the name "Mr. B". Similarly I may *understand* "there was a winged horse" although there never was one, because the statement means that, putting "fx" for "x has wings and is a horse", I assert "fx sometimes". It must be understood that "x" is not a constituent of "fx sometimes" or of "fx always". In fact, "x" means nothing. That is why beginners find it so hard to make out what it means.

When I infer something not experienced—whether I shall or shall not experience it hereafter—I am never inferring something that I can name, but only the truth of an existence-proposition. If induction is ever valid, it is possible to know existence-propositions without knowing any particular instance of their truth. Suppose, for instance, that A is a class of which we have experienced some members, and we infer that a member of A will occur. We have only to substitute "future memebers of A" for "members of A" to make our inference apply to a class of which we cannot mention any instance.

I incline to think that valid inductions, and, generally, inferences going beyond my personal past and present experience, always depend upon causation, sometimes supplemented by analogy. But, in the present article, I wished only to remove certain *a priori* objections to a certain kind of inference—objections which, though *a priori*, are urged by those who imagine themselves able to dispense with the *a priori* altogether.

There is, I think, a danger that logical positivism may develop a new kind of scholasticism, and may, by being unduly linguistic, forget the relation to fact that makes a statement true. I will give one illustration of what I should regard as scholasticism in the bad sense. Carnap, and others of the same school, have very rightly pointed out the confusions that arise when we do not distinguish adequately between using a word and naming it. In ordinary cases this danger does not arise. We say: Socrates was a man, "Socrates" is a word of eight letters. We can go on to say: "Socrates" is the name of a man, but ' "Socrates" ' is the name of a word. The usual method of designating a word by putting it in inverted commas is useful, presupposing that we know what a word is. But difficulties arise when we carry on the same process as regards sentences or propositions. If the words "Today is Tuesday" occur without quotes, the sentence is used, not named. But when the sentence occurs in quotes, what am I naming? Do I include sentences, in other languages, which have the same meaning, e. g. "aujourd'hui est mardi"? Conversely, suppose that in the Hottentot language the noise "today is Tuesday" means "I like cheese". Clearly this should not be included in what I designate by the phrase in quotes. We must say: a sentence in quotes designates the class of those utterances, in no matter what language, that have the significance that I attach to the sentence when I *use* it: we must include the Frenchman's phonetically dissimilar remark, and exclude the Hottentot's phonetically similar remark. It follows that we cannot tell what is designated by a sentence in quotes without first investigating what is meant by saying that two utterances have the same significance. At any rate, we cannot do so if the sentences, when used, are to be possible values of the

propositional variables used in logic, e. g. when we say "if *p* implies *q*, then not-*q* implies not-*p*". This shows that what is meant when a set of words is put in quotes is no such a simple matter as one would sometimes suppose from the works of some logical positivists. In this way, there is danger of a technique which conceals problems instead of helping to solve them.

Absorption in language sometimes leads to a neglect of the connection of language with non-linguistic facts, although it is this connection which gives meaning to words and significance to sentences. No one can understand the word "cheese" unless he has a non-linguistic acquaintance with cheese. The problem of meaning and significance requires much that is psychological, and demands some understanding of pre-linguistic mental processes. Most logical positivists fight shy of psychology, and therefore have little to say about meaning or significance. This makes them, in my opinion, somewhat narrow, and not capable of producing an all-round philosophy. They have, however, the great merit that their method allows them to tackle problems one by one, and that they are not obliged, as philosophers used to be, to produce a complete theory of the universe on all occasions. Their procedure, in fact, is more analogous to that of science than to that of traditional philosophy. In this respect I am wholly at one with them. I value their rigour, exactness, and attention to detail, and speaking broadly, I am more hopeful of results by methods such as theirs than by any that philosophers have employed in the past. What can be ascertained, can be ascertained by methods such as theirs; what cannot be ascertained by such methods we must be content not to know.

There is one matter of great philosophic importance in which a careful analysis of scientific inference and logical syntax leads—if I am not mistaken—to a conclusion which is unwelcome to me and (I believe) to almost all logical positivists. This conclusion is, that uncompromising empiricism is untenable. From a finite number of observations no general proposition can be inferred to be even probable unless we postulate some general principle of inference which cannot be established empirically. So far, there is agreement

among logical positivists. But as to what is to be done in consequence there is no agreement. Some hold that truth does not consist in conformity with fact, but only in coherence with other propositions already accepted for some undefined reason. Others, like Reichenbach, favour a posit which is a mere act of will and is admitted to be not intellectually justified. Yet others make attempts—to my mind futile—to dispense with general propositions. For my part, I assume that science is broadly speaking true, and arrive at the necessary postulates by analysis. But against the thorough-going sceptic I can advance no argument except that I do not believe him to be sincere.

Empiricism, Semantics, and Ontology

by Rudolf Carnap

1. *The Problem of Abstract Entities*

Empiricists are in general rather suspicious with respect to any kind of abstract entities like properties, classes, relations, numbers, propositions, etc. They usually feel much more in sympathy with nominalists than with realists (in the medieval sense). As far as possible they try to avoid any reference to abstract entities and to restrict themselves to what is sometimes called a nominalistic language, i. e., one not containing such references. However, within certain scientific contexts it seems hardly possible to avoid them. In the case of mathematics, some empiricists try to find a way out by treating the whole of mathematics as a mere calculus, a formal system for which no interpretation is given or can be given. Accordingly, the mathematician is said to speak not about numbers, functions, and infinite classes, but merely about meaningless symbols and formulas manipulated according to given formal rules. In physics it is more difficult to shun the suspected entities, because the language of physics serves for the communication of reports and predictions and hence cannot be taken as a mere calculus. A physicist who is suspicious of abstract entities may perhaps try to declare a certain part of the language of physics as uninterpreted and uninterpretable, that part which refers to real numbers as space-time coordinates or as values of physical magnitudes, to functions, limits, etc. More probably he will just speak about all these things like anybody else but with an uneasy conscience, like a man who in his everyday life does with qualms many things which are not in accord with the high

moral principles he professes on Sundays. Recently the problem of abstract entities has arisen again in connection with semantics, the theory of meaning and truth. Some semanticists say that certain expressions designate certain entities, and among these designated entities they include not only concrete material things but also abstract entities, e. g., properties as designated by predicates and propositions as designated by sentences [1]. Others object strongly to this procedure as violating the basic principles of empiricism and leading back to a metaphysical ontology of the Platonic kind.

It is the purpose of this article to clarify this controversial issue. The nature and implications of the acceptance of a language referring to abstract entities will first be discussed in general; it will be shown that using such a language does not imply embracing a Platonic ontology but is perfectly compatible with empiricism and strictly scientific thinking. Then the special question of the role of abstract entities in semantics will be dicussed. It is hoped that the clarification of the issue will be useful to those who would like to accept abstract entities in their work in mathematics, physics, semantics, or any other field; it may help them to overcome nominalistic scruples.

2. *Frameworks of Entities*

Are there properties, classes, numbers, propositions? In order to understand more clearly the nature of these and related problems, it is above all necessary to recognize a fundamental distinction between two kinds of questions concerning the existence or reality of entities. If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a *framework* for the new entities in question. And now we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind *within the framework*; we call them *internal questions*; and second, questions concerning the existence or reality *of*

[1] The terms "sentence" and "statement" are here used synonymously for declarative (indicative, propositional) sentences.

the framework itself, called *external questions*. Internal questions and possible answers to them are formulated with the help of the new forms of expressions. The answers may be found either by purely logical methods or by empirical methods, depending upon whether the framework is a logical or a factual one. An external question is of a problematic character which is in need of closer examination.

The world of things. Let us consider as an example the simplest framework dealt with in the everyday language: the spatio-temporally ordered system of observable things and events. Once we have accepted this thing-language and thereby the framework of things, we can raise and answer internal questions, e. g., "Is there a white piece of paper on my desk?", "Did King Arthur actually live?", "Are unicorns and centaurs real or merely imaginary?", and the like. These questions are to be answered by empirical investigations. Results of observations are evaluated according to certain rules as confirming or disconfirming evidence for possible answers. (This evaluation is usually carried out, of course, as a matter of habit rather than a deliberate, rational procedure. But it is possible, in a rational reconstruction, to lay down explicit rules for the evaluation. This is one of the main tasks of a pure, as distinguished from a psychological epistemology.) The concept of reality occurring in these internal questions is an empirical, scientific, non-metaphysical concept. To recognize something as a real thing or event means to succeed in incorporating it into the framework of things at a particular space-time position so that it fits together with the other things recognized as real, according to the rules of the framework.

From these questions we must distinguish the external question of the reality of the thing world itself. In contrast to the former questions, this question is raised neither by the man in the street nor by scientists, but only by philosophers. Realists give an affirmative answer, subjective idealists a negative one, and the controversy goes on for centuries without ever being solved. And it cannot be solved because it is framed in a wrong way. To be real in the scientific sense means to be an element of the framework; hence this concept cannot be meaningfully applied to the

framework itself. Those who raise the question of the reality of the thing world itself have perhaps in mind not a theoretical question as their formulation seems to suggest, but rather a practical question, a matter of a practical decision concerning the structure of our language. We have to make the choice whether or not to accept and use the forms of expression for the framework in question.

In the case of this particular example, there is usually no deliberate choice because we all have accepted the thing language early in our lives as a matter of course. Nevertheless, we may regard it as a matter of decision in this sense: we are free to choose to continue using the thing language or not; in the latter case we could restrict ourselves to a language of sense-data and other "phenomenal" entities, or construct an alternative to the customary thing language with another structure, or, finally, we could refrain from speaking. If someone decides to accept the thing language, there is no objection against saying that he has accepted the world of things. But this must not be interpreted as if it meant his acceptance of a *belief* in the reality of the thing world; there is no such belief or assertion or assumption, because it is not a theoretical question. To accept the thing world means nothing more than to accept a certain form of language, in other words, to accept rules for forming statements and for testing, accepting, or rejecting them. Thus the acceptance of the thing language leads, on the basis of observations made, also to the acceptance, belief, and assertion of certain statements. But the thesis of the reality of the thing world cannot be among these statements, because it cannot be formulated in the thing language or, it seems, in any other theoretical language.

The decision of accepting the thing language, althoug itself not of a cognitive nature, will nevertheless usually be influenced by theoretical knowledge, just like any other deliberate decision concerning the acceptance of linguistic or other rules. The purposes for which the language is intended to be used, for instance, the purpose of communicating factual knowledge, will determine which factors are relevant for the decision. The efficiency, fruitfulness, and simplicity of the use of the thing language may be among the decisive factors.

And the questions concerning these qualities are indeed of a theoretical nature. But these questions cannot be identified with the question of realism. They are not yes-no questions but questions of degree. The thing language in the customary form works indeed with a high degree of efficiency for most purposes of everyday life. This is a matter of fact, based upon the content of our experiences. However, it would be wrong to describe this situation by saying: "The fact of the efficiency of the thing language is confirming evidence for the reality of the thing world"; we should rather say instead: "This fact makes it advisable to accept the thing language".

The system of numbers. As an example of a framework which is of a logical rather than a factual nature let us take the system of natural numbers. This system is established by introducing into the language new expressions with suitable rules: (1) numerals like "five" and sentence forms like "there are five books on the table"; (2) The general term "number" for the new entities, and sentence forms like "five is a number"; (3) expressions for properties of numbers (e. g., "odd", "prime"), relations (e. g., "greater than"), and functions (e. g., "plus"), and sentence forms like "two plus three is five"; (4) numerical variables ("*m*","*n*", etc.) and quantifiers for universal sentences ("for every *n*, ...") and existential sentences ("there is an *n* such that ...") with the customary deductive rules.

Here again there are internal questions, e. g., "Is there a prime number greater than hundred?" Here, however, the answers are found, not by empirical investigation based on observations, but by logical analysis based on the rules for the new expressions. Therefore the answers are here analytic, i. e., logically true.

What is now the nature of the philosophical question concerning the existence or reality of numbers? To begin with, there is the internal question which, together with the affirmative answer, can be formulated in the new terms, say, by "There are numbers" or, more explicitly, "There is an *n* such that *n* is a number". This statement follows from the analytic statement "five is a number" and is therefore itself analytic. Moreover, it is rather trivial (in contradistinction to a statement like "There is a prime number greater than a

million", which is likewise analytic but far from trivial), because it does not say more than that the new system is not empty; but this is immediately seen from the rule which states that words like "five" are substitutable for the new variables. Therefore nobody who meant the question "Are there numbers?" in the internal sense would either assert or even seriously consider a negative answer. This makes it plausible to assume that those philosophers who treat the question of the existence of numbers as a serious philosophical problem and offer lengthy arguments on either side, do not have in mind the internal question. And, indeed, if we were to ask them: "Do you mean the question as to whether the system of numbers, *if* we were to accept it, would be found to be empty or not?", they would probably reply: "Not at all; we mean a question *prior* to the acceptance of the new framework". They might try to explain what they mean by saying that it is a question of the ontological status of numbers; the question whether or not numbers have a certain metaphysical characteristic called reality (but a kind of ideal reality, different from the material reality of the thing world) or subsistence or status of "independent entities". Unfortunately, these philosophers have so far not given a formulation of their question in terms of the common scientific language. Therefore our judgement must be that they have not succeeded in giving to the external question and to the possible answers any cognitive content. Unless and until they supply a clear cognitive interpretation, we are justified in our suspicion that their question is a pseudo-question, that is, one disguised in the form of a theoretical question while in fact it is non-theoretical; in the present case it is the practical problem whether or not to incorporate into the language the new linguistic forms which represent the framework of numbers.

The framework of propositions. New variables, "*p*", "*q*", etc., are introduced with a rule to the effect that any (declarative) sentence may be substituted for a variable of this kind; this includes, in addition to the sentences of the original thing language, also all general sentences with variables of any kind which may have been introduced into the language. Further, the general term "proposition" is

introduced. "*p* is a proposition" may be defined by "*p* or not *p*" (or by any other sentence form yielding only analytic sentences). Therefore, every sentence of the form "... is a proposition" (where any sentence may stand in the place of the dots) is analytic. This holds, for example, for the sentence:

(*a*) "Chicago is large is a proposition".

(We disregard here the fact that the rules of English grammar require not a sentence but a that-clause as the subject of another sentence; accordingly, instead of (*a*) we should have to say "That Chicago is large is a proposition".) Predicates may be admitted whose argument expressions are sentences; these predicates may be either extensional (e. g., the customary truth-functional connectives) or not (e. g., modal predicates like "possible", "necessary", etc.). With the help of the new variables, general sentences may be formed, e. g.

(*b*) "For every *p*, either *p* or not-*p*".

(*c*) "There is a *p* such that *p* is not necessary and not-*p* is not necessary".

(*d*) "There is a *p* such that *p* is a proposition".

(*c*) and (*d*) assert internal existence. The statement "There are propositions" may be meant in the sense of (*d*); in this case it is analytic (since it follows from (*a*)) and even trivial. If, however, the statement is meant in an external sense, then it is non-cognitive.

It is important to notice that the system of rules for the linguistic expressions of the propositional framework (of which only a few rules have here been briefly indicated) is sufficient for the introduction of the framework. Any further explanations as to the nature of the propositions (i. e., the elements of the framework indicated, the values of the variables "*p*", "*q*", etc.) are theoretically unnecessary because, if correct, they follow from the rules. For example, are propositions mental events (as in Russell's theory)? A look at the rules shows us that they are not, because otherwise existential statements would be of the form: "If the mental state of the person in question fulfils such and such conditions,

then there is a p such that ...". The fact that no references to mental conditions occur in existential statements (like (*c*), (*d*), etc.) shows that propositions are not mental entities. Further, a statement of the existence of linguistic entities (e. g., expressions, classes of expressions, etc.) must contain a reference to a language. The fact that no such reference occurs in the existential statements here, shows that propositions are not linguistic entities. The fact that in these statements no reference to a subject (an observer or knower) occurs (nothing like: "There is a p which is necessary for Mr. X"), shows that the propositions (and their properties, like necessity, etc.) are not subjective. Although characterizations of these or similar kinds are, strictly speaking, unnecessary, they may nevertheless be practically useful. If they are given, they should be understood, not as ingredient parts of the system, but merely as marginal notes with the purpose of supplying to the reader helpful hints or convenient pictorial associations which may make his learning of the use of the expressions easier than the bare system of the rules would do. Such a characterization is analogous to an extra-systematic explanation which a physicist sometimes gives to the beginner. He might, for example, tell him to imagine the atoms of a gas as small balls rushing around with great speed, or the electromagnetic field and its oscillations as quasi-elastic tensions and vibrations in an ether. In fact, however, all that can accurately be said about atoms or the field is implicitly contained in the physical laws of the theories in question [1].

[1] In my book *Meaning and Necessity* (Chicago, 1947) I have developed a semantical method which takes propositions as entities designated by sentences (more specifically, as intensions of sentences). In order to facilitate the understanding of the systematic development, I added some informal, extra-systematic explanations concerning the nature of propositions. I said that the term "proposition" "is used neither for a linguistic expression nor for a subjective, mental occurence, but rather for something objective that may or may not be exemplified in nature... We apply the term 'proposition' to any entities of a certain logical type, namely, those that may be expressed by (declarative) sentences in a language" (p. 27). After some more detailed discussions concerning the relation between propositions and facts, and the nature of false propositions, I added: "It has been the purpose of the preceding remarks to facilitate the understanding of our conception of propositions. If, however, a reader should find these explanations more puzzling than clarifying, or even unacceptable, he may disregard them" (p. 31) (that

The framework of thing properties. The thing language contains words like "red", "hard", "stone", "house", etc., which are used for describing what things are like. Now we may introduce new variables, say "f", "g", etc., for which those words are substitutable and furthermore the general term "property". New rules are laid down which admit sentences like "Red is a property", "Red is a color", "These two pieces of paper have at least one color in common" (i. e., "There is an f such that f is a color, and ..."). The last sentence is an internal asertion. It is of an empirical, factual nature. However, the external statement, the philosophical statement of the reality of properties—a special case of the thesis of the reality of universals—is devoid of cognitive content.

The frameworks of integers and rational numbers. Into a language containing the framework of natural numbers we may introduce first the (positive and negative) integers as relations among natural numbers and then the rational numbers as relations among integers. This involves introducing new types of variables, expressions substitutable for them and the general terms "integer" and " rational number".

The framework of real numbers. On the basis of the rational numbers, the real numbers may be introduced as classes of a special kind (segments) of rational numbers (according to the method developed by Dedekind and Frege). Here again a new type of variables is introduced, expressions substitutable for them (e. g., "$\sqrt{2}$"), and the general term "real number".

The framework of a spatio-temporal coordinate system for physics. The new entities are the space-time points.

is, disregard these extra-systematic explanations, not the whole theory of the propositions as intensions of sentences, as one reviewer understood). In spite of this warning, it seems that some of those readers who were puzzled by the explanations, did not disregard them but thought that by raising objections against them they could refute the theory. This is analogous to the procedure of some laymen who by (correctly) criticizing the ether picture or other visualizations of physical theories, thought they had refuted those theories. Perhaps the discussions in the present paper will help in clarifying the role of the system of linguistic rules for the introduction of a framework of entities on the one hand, and that of extra-systematic explanations concerning the nature of the entities on the other.

Each is an ordered quadruple of four real numbers, called its coordinates, consisting of three spatial and one temporal coordinates. The physical state of a spatio-temporal point or region is described either with the help of qualitative predicates (e. g., "hot") or by ascribing numbers as values of a physical magnitude (e. g., mass, temperature, and the like). The step from the framework of things (which does not contain space-time points but only extended objects with spatial and temporal relations between them) to the physical coordinate system is again a matter of decision. Our choice of certain features, although itself not theoretical, is suggested by theoretical knowledge, either logical or factual. For example, the choice of real numbers rather than rational numbers or integers as coordinates is not much influenced by the facts of experience but mainly due to considerations of mathematical simplicity. The restriction to rational coordinates would not be in conflict with any experimental knowledge we have, because the result of any measurement is a rational number. However, it would prevent the use of ordinary geometry (which says, e. g., that the diagonal of a square with the side 1 has the irrational value $\sqrt{2}$) and thus lead to great complications. On the other hand, the decision to use three rather than two or four spatial coordinates is strongly suggested, but still not forced upon us, by the result of common observations. If certain events allegedly observed in spiritualistic séances, e. g., a ball moving out of a sealed box, were confirmed beyond any reasonable doubt, it might seem advisable to use four spatial coordinates. Internal questions are here, in general, empirical questions to be answered by empirical investigations. On the other hand, the external questions of the reality of physical space and physical time are pseudo-questions. A question like "Are there (really) space-time points?" is ambiguous. It may be meant as an internal question; then the affirmative answer is, of course, analytic and trivial. Or it may be meant in the external sense: "Shall we introduce such and such forms into our language?"; in this case it is not a theoretical but a practical question, a matter of decision rather than assertion, and hence the proposed formulation would be misleading. Or finally, it may be meant in the following sense: "Are our experiences such that

the use of the linguistic forms in question will be expedient and fruitful?" This is a theoretical question of a factual, empirical nature. But it concerns a matter of degree; therefore a formulation in the form "real or not?" would be inadequate.

3. *What does Acceptance of a Framework mean?*

Let us now summarize the essential characteristics of situations involving the introduction of a new framework of entities, characterictics which are common to the various examples outlined above.

The acceptance of a framework of new entities is represented in the language by introduction of new forms of expressions to be used according to a new set of rules. There may be new names for particular entities of the kind in question; but some such names may already occur in the language before the introduction of the new framework. (Thus, for example, the thing language contains certainly words of the type of "blue" and "house" before the framework of properties is introduced; and it may contain words like "ten" in sentences of the form "I have ten fingers" before the framework of numbers is introduced.) The latter fact shows that the occurrence of constants of the type in question—regarded as names of entities of the new kind after the new framework is introduced—is not a sure sign of the acceptance of the framework. Therefore the introduction of such constants is not to be regarded as an essential step in the introduction of the framework. The two essential steps are rather the following. First, the introduction of a general term, a predicate of higher level, for the new kind of entities, permitting us to say of any particular entity that it belongs to this kind (e. g., "Red is a *property*", "Five is a *number*"). Second, the introduction of variables of the new type. The new entities are values of these variables; the constants (and the closed compound expressions, if any) are substitutable for the variables [1]. With the help of the variables, general sentences concerning the new entities can be formulated.

[1] W. V. Quine was the first to recognize the importance of the introduction of variables as indicating the acceptance of entities. "The

After the new forms are introduced into the language, it is possible to formulate with their help internal questions and possible answers to them. A question of this kind may be either empirical or logical; accordingly a true answer is either factually true or analytic.

From the internal questions we must clearly distinguish external questions, i. e., philosophical questions concerning the existence or reality of the framework itself. Many philosophers regard a question of this kind as an ontological question which must be raised and answered *before* the introduction of the new language forms. The latter introduction, they believe, is legitimate only if it can be justified by an ontological insight supplying an affirmative answer to the question of reality. In contrast to this view, we take the position that the introduction of the new ways of speaking does not need any theoretical justification because it does not imply any assertion of reality. We may still speak (and have done so) of "the acceptance of the framework" or "the acceptance of the new entities" since this form of speech is customary; but one must keep in mind that these phrases do not mean for us anything more than acceptance of the new linguistic forms. Above all, they must not be interpreted as referring to an assumption, belief, or assertion of "the reality of the entities". There is no such assertion. An alleged statement of the reality of the framework of entities is a pseudo-statement without cognitive content. To be sure, we have to face at this point an important question; but it is a practical, not a theoretical question; it is the question of whether or not to accept the new linguistic forms. The acceptance cannot be judged as being either true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended. Judgments of this

ontology to which one's use of language commits him comprises simply the objects that he treats as falling... within the range of values of his variables" ("Notes on Existence and Necessity", *Journal of Philos.*, 40 (1943), pp. 113-127, see p. 118; compare also his "Designation and Existence", *ibid.*, 36 (1939), pp. 701-9, and "On Universals", *Journal of Symbolic Logic*, 12 (1947), pp. 74-84).

kind supply the motivation for the decision of accepting or rejecting the framework. [1]

Thus it is clear that the acceptance of a framework must not be regarded as implying a metaphysical doctrine concerning the reality of the entities in question. It seems to me due to a neglect of this important distinction that some contemporary nominalists label the admission of variables of abstract types as "platonism". [2] This is, to say the least, an extremely misleading terminology. It leads to the absurd consequence, that the position of everybody who accepts the language of physics with its real number variables (as a language of communication, not merely as a calculus) would be called platonistic, even if he is a strict empiricist who rejects platonic metaphysics.

A brief historical remark may here be inserted. The non-cognitive character of the questions which we have called here external questions was recognized and emphasized already by the Vienna Circle under the leadership of Moritz Schlick, the group from which the movement of logical empiricism originated. Influenced by ideas of Ludwig Wittgenstein, the Circle rejected both the thesis of the reality

[1] For a closely related point of view on these questions see the detailed discussions in Herbert Feigl, *Existential Hypotheses*, forthcoming in *Philosophy of Science*, 1950.

[2] Paul Bernays, *Sur le platonisme dans les mathématiques* (*L'Enseignement math.*, 34 (1935), pp. 52-69). W. V. Quine, see footnote p. 65, and a recent paper *On What There Is*, (*Review of Metaphysics*, 2 (1948), pp. 21-38). Quine does not acknowledge the distinction which I emphasize above, because according to his general conception there are no sharp boundary lines between logical and factual truth, between questions of meaning and questions of fact, between the acceptance of a language structure ant the acceptance of an assertion formulated in the language. This conception, which seems to deviate considerably from customary ways of thinking, will be explained in his forthcoming book, *Foundations of Logic*. When Quine in the article mentioned above classifies my logicistic conception of mathematics (derived from Frege and Russell) as "platonic realism" (p. 33), this is meant (according to a personal communication from him) not as ascribing to me agreement with Plato's metaphysical doctrine of universals, but merely as referring to the fact that I accept a language of mathematics containing variables of higher levels. With respect to the basic attitude to take in choosing a language form (an "ontology" in Quine's terminology, which seems to me misleading), there appears now to be agreement between us: "the obvious counsel is tolerance and an experimental spirit" (*op. cit.*, p. 38).

of the external world and the thesis of its irreality as pseudo-statements;[1] the same was the case for both the thesis of the reality of universals (abstract entities, in our present terminology) and the nominalistic thesis that they are not real and that their alleged names are not names of anything but merely *flatus vocis*. (It is obvious that the apparent negation of a pseudo-statement must also be a pseudo-statement.) It is therefore not correct to classify the members of the Vienna Circle as nominalists, as is sometimes done. However, if we look at the basic anti-metaphysical and pro-scientific attitude of most nominalists (and the same holds for many materialists and realists in the modern sense), disregarding their occasional pseudo-theoretical formulations, then it is, of course, true to say that the Vienna Circle was much closer to those philosophers than to their opponents.

4. *Abstract Entities in Semantics*

The problem of the legitimacy and the status of abstract entities has recently again led to controversial discussions in connection with semantics. In a semantical meaning analysis certain expressions in a language are often said to designate (or name or denote or signify or refer to) certain extra-linguistic entities.[2] As long as physical things or events (e. g., Chicago or Caesar's death) are taken as designata (entities designated), no serious doubts arise. But strong objections have been raised, especially by some empiricists, against abstract entities as designata, e. g., against semantical statements of the following kind:

(1) "The word 'red' designates a property of things;"

[1] See Carnap, *Scheinprobleme in der Philosophie; das Fremdpsychische und der Realismusstreit*, Berlin, 1928. Moritz Schlick, *Positivismus und Realismus*, reprinted in *Gesammelte Aufsätze*, Wien 1938.

[2] See *Introduction to Semantics*, Cambridge Mass., 1942; *Meaning and Necessity*, Chicago, 1947. The distinction I have drawn in the latter book between the method of the name-relation and the method of intension and extension is not essential for our present discussion. The term "designation" is here used in a neutral way; it may be understood as referring to the name-relation or to the intension-relation or to the extension-relation or to any similar relations used in other semantical methods.

(2) "The word 'color' designates a property of properties of things;"

(3) "The word 'five' designates a number;"

(4) "The word 'odd' designates a property of numbers;"

(5) "The sentence 'Chicago is large' designates a proposition."

Those who criticize these statements do not, of course, reject the use of the expressions in question, like "red" or "five"; nor would they deny that these expressions are meaningful. But to be meaningful, they would say, is not the same as having a meaning in the sense of an entity designated. They reject the belief, which they regard as implicitly presupposed by those semantical statements, that to each expression of the types in question (adjectives like "red", numerals like "five", etc.) there is a particular real entity to which the expression stands in the relation of designation. This belief is rejected as incompatible with the basic principles of empiricism or of scientific thinking. Derogatory labels like "Platonic realism", "hypostatization", or " 'Fido'-Fido principle " are attached to it. The latter is the name given by Gilbert Ryle [1] to the criticized belief, which, in his view, arises by a naive inference of analogy: just as there is an entity well known to me, viz. my dog Fido, which is designated by the name "Fido", thus there must be for every meaningful expression a particular entity to which it stands in the relation of designation or naming, i. e., the relation exemplified by "Fido"-Fido. The belief criticized is thus a case of hypostatization, i. e., of treating as names expressions which are not names. While "Fido" is a name, expressions like "red", "five", etc. are said not to be names, not to designate anything.

Our previous discussions concerning the acceptance of frameworks enables us now to clarify the situation with respect to abstract entities as designata. Let us take as an example the statement:

(*a*) " 'Five' designates a number."

The formulation of this statement presupposes that our

[1] G. Ryle, *Meaning and Necessity* (*Philosophy*, 24 (1949), pp. 69-76).

language L contains the forms of expressions corresponding to what we have called the framework of numbers, in particular, numerical variables and the general term "number". If L contains these forms, the following is an analytic statement in L:

(*b*) "Five is a number."

Further, to make the statement (*a*) possible, L must contain an expression like "designates" or "is a name of" for the semantical relation of designation. If suitable rules for this term are laid down, the following is likewise analytic:

(*c*) "'Five' designates five."

(Generally speaking, any expression of the form "'...' designates..." is an analytic statement provided the term "..." is a constant in an accepted framework. If the latter condition is not fulfilled, the expression is not a statement.) Since (*a*) follows from (*c*) and (*b*), (*a*) is likewise analytic.

Thus it is clear that *if* someone accepts the framework of numbers, then he must acknowledge (*c*) and (*b*) and hence (*a*) as true statements. Generally speaking, if someone accepts a framework of entities, then he is bound to admit its entities as possible designata. Thus the question of the admissibility of entities of a certain type or of abstract entities in general as designata is reduced to the question of the acceptability of those entities. Both the nominalistic critics, who refuse the status of designators or names to expressions like "red", "five", etc., because they deny the existence of abstract entities, and the skeptics, who express doubts concerning the existence and demand evidence for it, treat the question of existence as a theoretical question. They do, of course, not mean the internal question; the affirmative answer to *this* question is analytic and trivial and too obvious for doubt or denial, as we have seen. Their doubts refer rather to the framework itself; hence they mean the external question. They believe that only after making sure that there really are entities of the kinds in question are we justified in accepting the framework by incorporating the linguistic forms into our language. However, we have seen that the external question is not a theoretical question but rather the practical question whether or not to accept those linguistic forms. This

acceptance is not in need of a theoretical justification (except with respect to expediency and fruitfulness), because it does not imply a belief or assertion. Ryle says that the "Fido"-Fido principle is "a grotesque theory". Grotesque or not, Ryle is wrong in calling it a theory. It is rather the practical decision to accept certain frameworks. Maybe Ryle is historically right with respect to those whom he mentions as previous representatives of the principle, viz. John Stuart Mill, Frege, and Russell. If these philosophers regarded the acceptance of a framework of entities as a theory, an assertion, they were victims of the same old, metaphysical confusion. But it is certainly wrong to regard *my* semantical method as involving a belief in the reality of abstract entities, since I reject a thesis of this kind as a metaphysical pseudo-statement.

The critics of the use of abstract entities in semantics overlook the fundamental difference between the acceptance of a framework of entities and an internal assertion, e. g., an assertion that there are elephants or electrons or prime numbers greater than a million. Whoever makes an internal assertion is certainly obliged to justify it by providing evidence, empirical evidence in the case of electrons, logical proof in the case of the prime numbers. The demand for a theoretical justification, correct in the case of internal assertions, is sometimes wrongly applied to the acceptance of a framework of entities. Thus, for example, Ernest Nagel [1] asks for "evidence relevant for affirming with warrant that there are such entities as infinitesimals or propositions". He characterizes the evidence required in these cases—in distinction to the empirical evidence in the case of electrons—as "in the broad sense logical and dialectical". Beyond this no hint is given as to what might be regarded as relevant evidence. Some nominalists regard the acceptance of abstract entities as a kind of superstition or myth, populating the world with fictitious or at least dubious entities, analogous to the belief in centaurs or demons. This shows again the confusion mentioned, because a superstition or myth is a false (or dubious) internal statement.

[1] E. Nagel, Review of Carnap *Meaning and Necessity* (*Journal of Philos.*, 45 (1948), pp. 467-72).

Let us take as example the natural numbers as cardinal numbers, i. e., in contexts like "Here are three books". The linguistic forms of the framework of numbers, including variables and the general term "number" are generally used in our common language of communication; and it is easy to formulate explicit rules for their use. Thus the logical characteristics of this framework are sufficiently clear (while many internal questions, i. e., arithmetical questions, are, of course, still open). In spite of this, the controversy concerning the external question of the ontological reality of numbers continues. Suppose that one philosopher says: "I believe that there are numbers as real entities. This gives me the right to use the linguistic forms of the numerical framework and to make semantical statements about numbers as designata of numerals". His nominalistic opponent replies: "You are wrong; there are no numbers. The numerals may still be used as meaningful expressions. But they are not names, there are no entities designated by them. Therefore the word "number" and numerical variables must not be used (unless a way were found to introduce them as merely abbreviating devices, a way of translating them into the nominalistic thing language)." I cannot think of any possible evidence that would be regarded as relevant by both philosophers, and therefore, if actually found, would decide the controversy or at least make one of the opposite theses more probable than the other. (To construe the numbers as classes or properties of the second level, according to the Frege-Russell method does, of course, not solve the controversy, because the first philosopher would affirm and the second deny the existence of classes or properties of the second level.) Therefore I feel compelled to regard the external question as a pseudo-question, until both parties to the controversy offer a common interpretation of the question as a cognitive question; this would involve an indication of possible evidence regarded as relevant by both sides.

There is a particular kind of misinterpretation of the acceptance of abstract entities in various fields of science and in semantics, that needs to be cleared up. Certain early British empiricists (e. g., Berkeley and Hume) denied the existence of abstract entities on the ground that immediate

experience presents us only with particulars, not with universals, e. g., with this red patch, but not with Redness or Color-in-General; with this scalene triangle, but not with Scalene Triangularity or Triangularity-in-General. Only entities belonging to a type of which examples were to be found within immediate experience could be accepted as ultimate constituents of reality. Thus, according to this way of thinking, the existence of abstract entities could be asserted only if one could show either that some abstract entities fall within the given, or that abstract entities can be defined in terms of the types of entity which are given. Since these empiricists found no abstract entities within the realm of sense-data, they either denied their existence, or else made a futile attempt to define universals in terms of particulars. Some contemporary philosophers, especially English philosophers following Bertrand Russell, think in basically similar terms. They emphasize a distinction between the data (that which is immediately given in consciousness, e. g. sense-data, immediately past experiences, etc.) and the constructs based on the data. Existence or reality is ascribed only to the data; the constructs are not real entities; the corresponding linguistic expressions are merely ways of speech not actually designating anything (reminiscent of the nominalists' *flatus vocis*). We shall not criticize here this general conception. (As far as it is a principle of accepting certain entities and not accepting others, leaving aside any ontological, phenomenalistic and nominalistic pseudo-statements, there cannot be any theoretical objection to it.) But if this conception leads to the view that other philosophers or scientists who accept abstract entities thereby assert or imply their occurrence as immediate data, then such a view must be rejected as a misinterpretation. References to space-time points, the electromagnetic field, or electrons in physics, to real or complex numbers and their functions in mathematics, to the excitatory potential or unconscious complexes in psychology, to an inflationary trend in economics, and the like, do not imply the assertion that entities of these kinds occur as immediate data. And the same holds for references to abstract entities as designata in semantics. Some of the criticisms by English philosophers against such references give the im-

pression that, probably due to the misinterpretation just indicated, they accuse the semanticist not so much of bad metaphysics (as some nominalists would do) but of bad psychology. The fact that they regard a semantical method involving abstract entities not merely as doubtful and perhaps wrong, but as manifestly absurd, preposterous and grotesque, and that they show a deep horror and indignation against this method, is perhaps to be explained by a misinterpretation of the kind described. In fact, of course, the semanticist does not in the least assert or imply that the abstract entities to which he refers can be experienced as immediately given either by sensation or by a kind of rational intuition. An assertion of this kind would indeed be very dubious psychology. The psychological question as to which kinds of entities do and which do not occur as immediate data is entirely irrelevant for semantics, just as it is for physics, mathematics, economics, etc., with respect to the examples mentioned above.[1]

5. *Conclusion*

For those who want to develop or use semantical methods, the decisive question is not the alleged ontological question of the existence of abstract entities but rather the question whether the use of abstract linguistic forms or, in technical terms, the use of variables beyond those for things (or phenomenal data), is expedient and fruitful for the purposes for which semantical analyses are made, viz. the analysis, interpretation, clarification, or construction of languages of communication, especially languages of science. This question is here neither decided nor even discussed. It is not a question simply of yes or no, but a matter of degree. Among those philosophers who have carried out semantical analyses and thought about suitable tools for this work, beginning with Plato and Aristotle and, in a more technical way on the basis of modern logic, with C. S. Pierce and Frege, a great majority accepted abstract entities. This does, of course, not prove the

[1] Wilfrid Sellars (*Acquaintance and Description Again*, in *Journal of Philos.* 46 (1949), pp. 496-504, see pp. 502 f.) analyzes clearly the roots of the mistake "of taking the designation relation of semantic theory to be a reconstruction of *being present to an experience*".

case. After all, semantics in the technical sense is still in the initial phases of its development, and we must be prepared for possible fundamental changes in methods. Let us therefore admit that the nominalistic critics may possibly be right. But if so, they will have to offer better arguments than they did so far. Appeal to ontological insight will not carry much weight. The critics will have to show that it is possible to construct a semantical method which avoids all references to abstract entities and achieves by simpler means essentially the same results as the other methods.

The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. To decree dogmatic prohibitions of certain linguistic forms instead of testing them by their success or failure in practical use, is worse than futile; it is positively harmful because it may obstruct scientific progress. The history of science shows examples of such prohibitions based on prejudices deriving from religious, mythological, metaphysical, or other irrational sources, which slowed up the developments for shorter or longer periods of time. Let us learn from the lessons of history. Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. *Let us be cautious in making assertions and critical in examining them, but tolerant in permitting linguistic forms.*

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Problems and Changes in the Empiricist Criterion of Meaning

by Carl G. Hempel

1. *Introduction*

The fundamental tenet of modern empiricism is the view that all non-analytic knowledge is based on experience. Let us call this thesis the principle of empiricism.[1] Contemporary logical empiricism has added[2] to it the maxim that a sentence makes a cognitively meaningful assertion, and thus can be said to be either true or false, only if it is either (1) analytic or self-contradictory or (2) capable, at least in principle, of experiential test. According to this so-called *empiricist criterion of cognitive meaning, or of cognitive significance*, many of the formulations of traditional metaphysics and large parts of epistemology are devoid of cognitive significance—however rich some of them may be in non-cognitive import by virtue of their emotive appeal or the moral inspiration they offer. Similarly certain doctrines which have been, at one time or another, formulated within empirical science or its border disciplines are so contrived as to be incapable of test by any conceivable evidence; they are therefore qualified as pseudo-

[1] This term is used by Benjamin (2) in an examination of the foundations of empiricism. For a recent discussion of the basic ideas of empiricism see Russell (27), Part Six.

[2] In his stimulating article, Positivism, W. T. Stace argues, in effect, that the testability criterion of meaning is not logically entailed by the principle of empiricism. (See (29), especially section 11.) This is correct: According to the latter, a sentence expresses knowledge only if it is either analytic or corroborated by empirical evidence; the former goes further and identifies the domain of cognitively significant discourse with that of potential knowledge; i. e., it grants cognitive import only to sentences for which—unless they are either analytic or contradictory—a test by empirical evidence is conceivable.

hypotheses, which assert nothing, and which therefore have no explanatory or predictive force whatever. This verdict applies, for example, to the neo-vitalist speculations about entelechies or vital forces, and to the "telefinalist hypothesis" propounded by Lecomte du Noüy. [1]

The preceding formulations of the principle of empiricism and of the empiricist meaning criterion provide no more, however, than a general and rather vague characterization of a basic point of view, and they need therefore to be elucidated and amplified. And while in the earlier phases of its development, logical empiricism was to a large extent preoccupied with a critique of philosophic and scientific formulations by means of those fundamental principles, there has been in recent years an increasing concern with the positive tasks of analyzing in detail the logic and methodology of empirical science and of clarifying and restating the basic ideas of empiricism in the light of the insights thus obtained. In the present article, I propose to discuss some of the problems this search has raised and some of the results it seems to have established.

2. *Changes in the testability criterion of empirical meaning*

As our formulation shows, the empiricist meaning criterion lays down the requirement of experiential testability for those among the cognitively meaningful sentences which are neither analytic nor contradictory; let us call them sentences with empirical meaning, or empirical significance. The concept of testability, which is to render precise the vague notion of being based—or rather baseable—on experience, has undergone several modifications which reflect an increasingly refined analysis of the structure of empirical knowledge. In the present section, let us examine the major stages of this development.

For convenience of exposition, we first introduce three auxiliary concepts, namely those of observable characteristic, of observation predicate, and of observation sentence. A property or a relation of physical objects will be called an *observable characteristic* if, under suitable circumstances, its presence or absence in a given instance can be ascertained

[1] Cf. (19), Ch. XVI.

through direct observation. Thus, the terms "green", "soft", "liquid", "longer than", designate observable characteristics, while "bivalent", "radioactive", "better electric conductor", and "introvert" do not. Terms which designate observable characteristics will be called *observation predicates*. Finally, by an *observation sentence* we shall understand any sentence which—correctly or incorrectly—asserts of one or more specifically named objects that they have, or that they lack, some specified observable characteristic. The following sentences, for example, meet this condition: "The Eiffel Tower is taller than the buidings in its vicinity", "The pointer of this instrument does not cover the point marked '3' on the scale", and even, "The largest dinosaur on exhibit in New York's Museum of Natural History had a blue tongue"; for this last sentence assigns to a specified object a characteristic—having a blue tongue—which is of such a kind that under suitable circumstances (e.g., in the case of my Chow dog) its presence or absence can be ascertained by direct observation. Our concept of observation sentence is intended to provide a precise interpretation of the vague idea of a sentence asserting something that is "in principle" ascertainable by direct observation, even though it may happen to be actually incapable of being observed by myself, perhaps also by my contemporaries, and possibly even by any human being who ever lived or will live. Any evidence that might be adduced in the test of an empirical hypothesis may now be thought of as being expressed in observation sentences of this kind.[1]

We now turn to the changes in the conception of testability, and thus of empirical meaning. In the early days of the Vienna Circle, a sentence was said to have empirical

[1] Observation sentences of this kind belong to what Carnap has called the thing-language (cf., e. g., (7), pp. 52-53). That they are adequate to formulate the data which serve as the basis for empirical tests is clear in particular for the intersubjective testing procedures used in science as well as in large areas of empirical inquiry on the common-sense level. In epistemological discussions, it is frequently assumed that the ultimate evidence for beliefs about empirical matters consists in perceptions and sensations whose description calls for a phenomenalistic type of language. The specific problems connected with the phenomenalistic approach cannot be discussed here; but it should be mentioned that at any rate all the critical considerations presented in this article in regard to the testability criterion are applicable, *mutatis mutandis*, to the case of a phenomenalistic basis as well.

meaning if it was capable, at least in principle, of complete verification by observational evidence; i.e., if observational evidence could be described which, if actually obtained, would conclusively establish the truth of the sentence.[1] With the help of the concept of observation sentence, we can restate this requirement as follows: A sentence S has empirical meaning if and only if it is possible to indicate a finite set of observation sentences, O_1, O_2, ..., O_n, such that if these are true, then S is necessarily true, too. As stated, however, this condition is satisfied also if S is an analytic sentence or if the given observation sentences are logically incompatible with each other. By the following formulation, we rule these cases out and at the same time express the intended criterion more precisely:

[1] Originally, the permissible evidence was meant to be restricted to what is observable by the speaker and perhaps his fellow-beings during their life times. Thus construed, the criterion rules out, as cognitively meaningless, all statements about the distant future or the remote past, as has been pointed out, among others, by Ayer in (1), Chapter I; by Pap in (21), Chapter 13, esp. pp. 333 ff.; and by Russell in (27), pp. 445-447. This difficulty is avoided, howerer, if we permit the evidence to consist of any finite set of "logically possible observation data", each of them formulated in an observation sentence. Thus, e. g., the sentence S_1, "The tongue of the largest dinosaur in New York's Museum of Natural History was blue or black" is completely verifiable in our sense; for it is a logical consequence of the sentence S_2, "The tongue of the largest dinosaur in New York's Museum of Natural History was blue"; and this is an observation sentence, as has been shown above.

And if the concept of *verifiability in principle* and the more general concept of *confirmability in principle*, which will be considered later, are construed as referring to *logically possible evidence* as expressed by observation sentences, then it follows similarly that the class of statements which are verifiable, or at least confirmable, in principle includes such assertions as that the planet Neptune and the Antarctic Continent existed before they were discovered, and that atomic warfare, if not checked, may lead to the extermination of this planet. The objections which Russell (cf. (27), pp. 445 and 447) raises against the verifiability criterion by reference to those examples do not apply therefore if the criterion is understood in ther manner here suggested. Incidentally, statements of the kind mentioned by Russell, which are not actually verifiable by any human being, were explicitly recognized as cognitively significant already by Schlick (in (28), Part V), who argued that the impossibility of verifying them was "merely empirical". The characterization of verifiability with the help of the concept of observation sentence as suggested here might serve as a more explicit and rigorous statement of that conception.

(2.1) *Requirement of complete verifiability in principle:* A sentence has empirical meaning if and only if it is not analytic and follows logically from some finite and logically consistent class of observation sentence.[1]

This criterion, however, has several serious defects. The

[1] As has frequently been emphasized in empiricist literature, the term "verifiability" is to indicate, of course, the conceivability, or better, the logical possibility of evidence of an observational kind which, if actually encountered, would constitute conclusive evidence for the given sentence; it is not intended to mean the technical possibility of performing the tests needed to obtain such evidence, and even less does it mean the possibility of actually finding directly observable phenomena which constitute conclusive evidence for that sentence—which would be tantamount to the actual existence of such evidence and would thus imply the truth of the given sentence. Analogous remarks apply to the terms "falsifiability" and "confirmability". This point has been disregarded in some recent critical discussions of the verifiability criterion. Thus, e. g., Russell (cf. (27), p. 448) construes verifiability as the actual existence of a set of conclusively verifying occurrences. This conception, which has never been advocated by any logical empiricist, must naturally turn out to be inadequate since according to it the empirical meaningfulness of a sentence could not be established without gathering empirical evidence, and moreover enough of it to permit a conclusive proof of the sentences in question! It is not surprising, therefore, that his extraordinary interpretation of verifiability leads Russell to the conclusion: "In fact, that a proposition is verifiable is itself not verifiable" (*l. c.*) Actually, under the empiricist interpretation of complete verifiability, any statement asserting the verifiability of some sentence S whose text is quoted, is either analytic or contradictory; for the decision whether there exists a class of observation sentences which entail S, i. e., whether such observation sentences can be formulated, no matter whether they are true or false—that decision is a matter of pure logic and requires no factual information whatever.

A similar misunderstanding is in evidence in the following passage in which W. H. Werkmeister claims to characterize a view held by logical positivists: "A proposition is said to be 'true' when it is 'verifiable in principle'; i. e., when we know the conditions which, when realized, will make 'verification' possible (cf. Ayer)." (cf. (31), p. 145). The quoted thesis, which, again, was never held by any logical positivist, including Ayer, is in fact logically absurd. For we can readily describe conditions which, if realized, would verify the sentence "The outside of the Chrysler Building is painted a bright yellow"; but similarly, we can describe verifying conditions for its denial; hence, according to the quoted principle, both the sentence and its denial would have to be considered true. Incidentally, the passage under discussion does not accord with Werkmeister's perfectly correct observation, *l. c.*, p. 40, that verifiability is intended to characterize the meaning of a sentence—which shows that verifiability is meant to be a criterion of cognitive signifiance rather than of truth.

first of those here to be mentioned has been pointed out by various writers:

(*a*) The verifiability requirement rules out all sentences of universal form and thus all statements purporting to express general laws; for these cannot be conclusively verified by any finite set of observational data. And since sentences of this type constitute an integral part of scientific theories, the verifiability requirement must be regarded as overly restrictive in this respect. Similarly, the criterion disqualifies all sentences such as "For any substance there exists some solvent", which contain both universal and existential quantifiers (i.e., occurrences of the terms "all" and "some" or their equivalents); for no sentences of this kind can be logically deduced from any finite set of observation sentences.

Two further defects of the verifiability requirement do not seem to have been widely noticed:

(*b*) Suppose that S is a sentence which satisfies the proposed criterion, whereas N is a sentence such as "The absolute is perfect", to which the criterion attributes no empirical meaning. Then the alternation SvN (i.e., the expression obtained by connecting the two sentences by the word "or"), likewise satisfies the criterion; for if S is a consequence of some finite class of observation sentences, then trivially SvN is a consequence of the same class. But clearly, the empiricist criterion of meaning is not intended to countenance sentences of this sort. In this respect, therefore, the requirement of complete verifiability is too inclusive.

(*c*) Let "P" be an observation predicate. Then the purely existential sentence "$(\mathrm{E}x)\mathrm{P}(x)$" ("There exists at least one thing that has the property P") is completely verifiable, for it follows from any observation sentence asserting of some particular object that it has the property P. But its denial, being equivalent to the universal sentence "$(x) \sim \mathrm{P}(x)$" ("Nothing has the property P") is clearly not completely verifiable, as follows from comment (*a*) above. Hence, under the criterion (2.1), the denials of certain empirically—and thus cognitively—significant sentences are empirically meaningless; and as they are neither analytic nor contradictory, they are cognitively meaningless. But however we may

delimit the domain of significant discourse, we shall have to insist that if a sentence falls within that domain, then so must its denial. To put the matter more explicitly: The sentences to be qualified as cognitively meaningful are precisely those which can be significantly said to be either true or false. But then, adherence to (2.1) would engender a serious dilemma, as is shown by the consequence just mentioned: We would either have to give up the fundamental logical principle that if a sentence is true or false, then its denial is false or true, respectively (and thus cognitively significant); or else, we must deny, in a manner reminiscent of the intuitionistic conception of logic and mathematics, that "$(x) \sim P(x)$" is logically equivalent to the negation of "$(Ex)\ P\ (x)$". Clearly, the criterion (2.1), which has disqualified itself on several other counts, does not warrant such drastic measures for its preservation; hence, it has to be abandoned.[1]

Strictly analogous considerations apply to an alternative criterion, which makes complete falsifiability in principle the defining characteristic of empirical significance. Let us formulate this criterion as follows: A sentence has empirical meaning if and only if it is capable, in principle, of complete refutation by a finite number of observational data; or, more precisely:

(2.2) *Requirement of complete falsifiability in principle:*

A sentence has empirical meaning if and only if its denial is

[1] The arguments here adduced against the verifiability criterion also prove the inadequacy of a view closely related to it, namely that two sentences have the same cognitive significance if any set of observation sentences which would verify one of them would also verify the other, and conversely. Thus, e. g., under this criterion, any two general laws would have to be assigned the same cognitive significance, for no general law is verified by any set of observation sentences. The view just referred to must be clearly distinguished from a position which Russell examines in his critical discussion of the positivistic meaning criterion. It is "the theory that two propositions whose verified consequences are identical have the same significance" ((27), p. 448). This view is untenable indeed, for what consequences of a statement have actually been verified at a given time is obviously a matter of historical accident which cannot possibly serve to establish identity of cognitive significance. But I am not aware that any logical positivist ever subscribed to that "theory".

not analytic and follows logically from some finite logically consistent class of observation sentences.[1]

This criterion qualifies a sentence as empirically meaningful if its denial satisfies the requirement of complete verifiability; as is to be expected, it is therefore inadequate on similar grounds as the latter:

(*a*) It rules out purely existential hypotheses, such as "There exists at least one unicorn", and all sentences whose formulation calls for mixed—i.e., universal and existential—quantification; for none of these can possibly be conclusively falsified by a finite number of observation sentences.

(*b*) If a sentence S is completely falsifiable whereas N is a sentence which is not, then their conjunction, S.N (i.e., the expression obtained by connecting the two sentences by the word "and") is completely falsifiable; for if the denial of S is entailed by some class of observation sentences, then the denial of S.N is, *a fortiori*, entailed by the same class. Thus, the criterion allows empirical signifiance to many sentences which an adequate empiricist criterion should rule out, such as, say "All swans are white and the absolute is perfect."

(c) If "P" is an observation predicate, then the assertion that all things have the property P is qualified as significant, but its denial, being equivalent to a purely existential hypothesis, is disqualified (cf. (*a*)). Hence, criterion (2.2) gives rise to the same dilemma as (2.1).

In sum, then, interpretations of the testability criterion in terms of complete verifiability or of complete falsifiability are inadequate because they are overly restrictive in one direction and overly inclusive in another, and because both of them require incisive changes in the fundamental principles of logic.

Several attempts have been made to avoid these difficulties by construing the testability criterion as demanding merely a partial and possibly indirect confirmability of empirical hypotheses by observational evidence.

[1] The idea of using theoretical falsifiability by observational evidence as the "criterion of demarcation" separating empirical science from mathematics and logic on the one hand and from metaphysics on the other is due to K. Popper (cf. (22), section 1-7 and 19-24; also see (23), vol. II, pp. 282-285). Whether Popper would subscribe to the proposed restatement of the falsifiability criterion, I do not know.

(2.3) A formulation suggested by Ayer [1] is characteristic of these attempts to set up a clear and sufficiently comprehensive criterion of confirmability. It states, in effect, that a sentence S has empirical import if from S in conjunction with suitable subsidiary hypotheses it is possible to derive observation sentences which are not derivable from the subsidiary hypotheses alone.

This condition is suggested by a closer consideration of the logical structure of scientific testing; but it is much too liberal as it stands. Indeed, as Ayer himself has pointed out in the second edition of his book, *Language, Truth, and Logic*, [2] his criterion allows empirical import to any sentence whatever. Thus, e.g., if S is the sentence "The absolute is perfect", it suffices to choose as a subsidiary hypothesis the sentence "If the absolute is perfect then this apple is red" in order to make possible the deduction of the observation sentence "This apple is red," which clearly does not follow from the subsidiary hypothesis alone. [3]

[1] (1), Ch. I.—The case against the requirements of verifiability and of falsifiability, and favor of a requirement of partial confirmability and disconfirmability is very clearly presented also by Pap in (21), Chapter 13.

[2] (1), 2d ed., pp. 11-12.

[3] According to Stace (cf. (29), p. 218), the criterion of partial and indirect testability, which he calls the positivist principle, presupposes (and thus logically entails) another principle, which he terms the *Principle of Observable Kinds*: "A sentence, in order to be significant, must assert or deny facts which are of a kind or class such that it is logically possible directly to observe some facts which are instances of that class or kind. And if a sentence purports to assert or deny facts which are of a class or kind such that it would be logically impossible directly to observe any instance of that class or kind, then the sentence is non-significant." I think the argument Stace offers to prove that this principle is entailed by the requirement of testability is inconclusive (mainly because of the incorrect tacit assumption that "on the transformation view of deduction", the premises of a valid deductive argument must be necessary conditions for the conclusion (*l. c.*, p. 225)). Without pressing this point any further, I should like to add here a remark on the principle of observable kinds itself. Professor Stace does not say how we are to determine what "facts" a given sentence asserts or denies, or inded whether it asserts or denies any "facts" at all. Hence, the exact import of the principle remains unclear. No matter, however, how one might choose the criteria for the factual reference of sentences, this much seems certain: If a sentence expresses any fact at all, say *f*, then it satisfies the requirement laid down in the first sentence of the principle; for we can always form a class containing *f* together with the fact expressed by some observation

(2.4) To meet this objection, Ayer has recently proposed a modified version of his testability criterion. The modification restricts, in effect, the subsidiary hypotheses mentioned in (2.3) to sentences which are either analytic or can independently be shown to be testable in the sense of the modified criterion.[1]

But it can readily be shown that this new criterion, like the requirement of complete falsifiability, allows empirical significance to any conjunction S.N, where S satisfies Ayer's criterion while N is a sentence such as "The absolute is perfect," which is to be disqualified by that criterion. Indeed: whatever consequences can be deduced from S with the help of permissible subsidiary hypotheses can also de deduced from S.N by means of the same subsidiary hypotheses, and as Ayer's new criterion is formulated essentially in terms of the deducibility of a certain type of consequence from the given sentence, it countenances S.N together with S. Another difficulty has been pointed out by Professor A. Church, who has shown[2] that if there are any three observation sentences none of which alone entails any of the others, then it follows for any sentence S whatsoever that either it or its denial has empirical import according to Ayer's revised criterion.

3. *Translatability into an empiricist language as a new criterion of cognitive meaning*

I think it is useless to continue the search for an adequate criterion of testability in terms of deductive relationships to observation sentences. The past development of this search—of which we have considered the major stages—seems to warrant the expectation that as long as we try to set up a criterion of testability for individual sentences in a natural language, in terms of logical relationship to observation sentences, the result will be either too restrictive or too inclusive,

sentence of our choice, which makes *f* a member of a class of facts at least one of which is capable, in principle, of direct observation. The first part of the principle of observable kinds is therefore all-inclusive, somewhat like Ayer's original formulation of the empiricist meaning criterion.

[1] This restriction is expressed in recursive form and involves no vicious circle. For the full statement of Ayers's criterion, see (1), 2d edition, p. 13.

[2] Church (11).

or both. In particular it appears likely that such criteria would allow empirical import, in the manner of (2.1)(*b*) or of (2.2)(*b*), either to any alternation or to any conjunction of two sentences of which at least one is qualified as empirically meaningful; and this peculiarity has undesirable consequences because the liberal grammatical rules of English as of any other natural language countenance as sentences certain expressions ("The absolute is perfect" was our illustration) which even by the most liberal empiricist standards make no assertion whatever; and these would then have to be permitted as components of empirically significant statements.

The predicament would not arise, of course, in an artificial language whose vocabulary and grammar were so chosen as to preclude altogether the possibility of forming sentences of any kind which the empiricist meaning criterion is intended to rule out. Let us call any such language an *empiricist language*. This reflection suggests an entirely different approach to our problem: Give a general characterization of the kind of language that would qualify as empiricist, and then lay down the following

(3.1) *Translatability criterion of cognitive meaning:* A sentence has cognitive meaning if and only if it is translatable into an empiricist language.

This conception of cognitive import, while perhaps not explicitly stated, seems to underlie much of the more recent work done by empiricist writers; as far as I can see it has its origin in Carnap's essay, *Testability and Meaning* (especially part IV).

As any language, so also any empiricist language can be characterized by indicating its vocabulary and the rules determining its logic; the latter include the syntactical rules according to which sentences may be formed by means of the given vocabulary. In effect, therefore, the translatability criterion proposes to characterize the cognitively meaningful sentences by the vocabulary out of which they may be constructed, and by the syntactical principles governing their construction. What sentences are singled out as cognitively significant will depend, accordingly, on the choice of the vocabulary and of the construction rules. Let us consider a specific possibility:

(3.2) We might qualify a language L as empiricist if it satisfies the following conditions:

(a) *The vocabulary of L* contains:

(1) The customary locutions of logic which are used in the formulation of sentences; including in particular the expressions "not", "and", "or", "if... then...", "all", "some", "the class of all things such that...", "... is an element of class...";

(2) Certain *observation predicates*. These will be said to constitute the basic empirical vocabulary of L;

(3) Any expression definable by means of those referred to under (1) and (2).

(b) *The rules of sentence formation for L* are those laid down in some contemporary logical system such as *Principia Mathematica.*

Since all defined terms can be eliminated in favor of primitives, these rules stipulate in effect that a language L is empiricist if all its sentences are expressible, with the help of the usual logical locutions, in terms of observable characteristics of physical objects. Let us call any language of this sort a thing-language in the narrower sense. Alternatively, the basic empirical vocabulary of an empiricist language might be construed as consisting of phenomenalistic terms, each of them referring to some aspect of the phenomena of perception or sensation. The construction of adequate phenomenalistic languages, however, presents considerable difficulties [1], and in recent empiricism, attention has been focussed primarily on the potentialities of languages whose basic empirical vocabulary consists of observation predicates; for the latter lend themselves more directly to the description of that type of intersubjective evidence which is invoked in the test of scientific hypotheses.

If we construe empiricist languages in the sense of (3.2), then the translatability criterion (3.1) avoids all of the shortcomings pointed out in our discussion of earlier forms of the testability criterion:

[1] Important contributions to the problem have been made by Carnap (5) and by Goodman (15).

(*a*) Our characterization of empiricist languages makes explicit provision for universal and existential quantification, i.e., for the use of the terms "all" and "some"; hence, no type of quantified statement is generally excluded from the realm of cognitively significant discourse;

(*b*) Sentences such as "The absolute is perfect" cannot be formulated in an empiricist language (cf. (*d*) below); hence there is no danger that a conjunction or alternation containing a sentence of that kind as a component might be qualified as cognitively significant;

(*c*) In a language L with syntactical rules conforming to *Principia Mathematica*, the denial of a sentence is always again a sentence of L. Hence, the translatability criterion does not lead to the consequence, which is entailed by both (2.1) and (2.2), that the denials of certain significant sentences are non-significant;

(*d*) Despite its comprehensiveness, the new criterion does not attribute cognitive meaning to *all* sentences; thus, e.g., the sentences "The absolute is perfect" and "Nothingness nothings" cannot be translated into an empiricist language because their key terms are not definable by means of purely logical expressions and observation terms.

4. *The problem of disposition terms and of theoretical constructs*

Yet, the new criterion is still too restrictive—as are, incidentally, also its predecessors—in an important respect which now calls for consideration. If empiricist languages are defined in accordance with (3.2), then, as was noted above, the translatability criterion (3.1) allows cognitive import to a sentence only if its constitutive empirical terms are explicitly definable by means of observation predicates. But as we shall argue presently, many terms even of the physical sciences are not so definable; hence the criterion would oblige us to reject, as devoid of cognitive import, all scientific hypotheses containing such terms—an altogether intolerable consequence.

The concept of temperature is a case in point. At first glance, it seems as though the phrase "Object x has a temperature of c degrees centigrade", or briefly "$T(x)=c$" could

be defined by the following sentence, (D): $T(x) = c$ if and only if the following condition is satisfied: If a thermometer is in contact with x, then it registers c degrees on its scale.

Disregarding niceties, it may be granted that the definiens given here is formulated entirely in reference to observables. However, it has one highly questionable aspect: In *Principia Mathematica* and similar systems, the phrase "if p then q" is construed as being synonymous with "not p or q"; and under this so-called material interpretation of the conditional, a statement of the form "if p then q" is obviously true if (though not only if) the sentence standing in the place of "p" is false. If, therefore, the meaning of "if... then..." in the definiens of (D) is understood in the material sense, then that definiens is true if (though not only if) x is an object not in contact with a thermometer—no matter what numerical value we may give to c. And since the definiendum would be true under the same circumstances, the definition (D) would qualify as true the assignment of any temperature value whatsoever to any object not in contact with a thermometer! Analogous considerations apply to such terms as "electrically charged", "magnetic", "intelligent", "electric resistance", etc., in short to all disposition terms, i.e., terms which express the disposition of one or more objects to react in a determinate way under specified circumstances: A definition of such terms by means of observation predicates cannot be effected in the manner of (D), however natural and obvious a mode of definition this may at first seem to be.[1]

There are two main directions in which a resolution of the difficulty might be sought. On the one hand, it could be argued that the definition of disposition terms in the manner of (D) is perfectly adequate provided that the phrase "if... then..." in the definiens is construed in the sense it is obviously intended to have, namely as implying, in the case of (D), that even if x is not actually in contact with a thermometer, still if it *were* in such contact, then the thermometer *would* register c degrees. In sentences such as this, the phrase "if... then..." is said to be used counterfactually; and it is in this "strong" sense, which implies a counterfactual conditional,

[1] This difficulty in the definition of disposition terms was first pointed out and analyzed by Carnap (in (6); see esp. section 7).

that the definiens of (D) would have to be construed. This suggestion would provide an answer to the problem of defining disposition terms if it were not for the fact that no entirely satisfactory account of the exact meaning of counterfactual conditionals seems to be available at present. Thus, the first way out of the difficulty has the status of a program rather than that of a solution. The lack of an adequate theory of counterfactual conditionals is all the more deplorable as such a theory is needed also for the analysis of the concept of general law in empirical science and of certain related ideas. A clarification of this cluster of problems constitutes at present one of the urgent desiderata in the logic and methodology of science. [1]

An alternative way of dealing with the definitional problems raised by disposition terms was suggested, and developed in detail, by Carnap. It consists in permitting the introduction of new terms, within an empiricist language, by means of so-called reduction sentences, which have the character of partial or conditional definitions. [2] Thus, e.g., the concept of temperature in our last illustration might be introduced by means of the following reduction sentence, (R): If a thermometer is in contact with an object x, then $T(x) = c$ if and only if the thermometer registers c degrees.

This rule, in which the conditional may be construed in the material sense, specifies the meaning of "temperature", i.e., of statements of the form "$T(x) = c$", only partially, namely in regard to those objects which are in contact with a thermometer; for all other objects, it simply leaves the meaning of "$T(x) = c$" undetermined. The specification of the meaning

[1] The concept of strict implication as introduct by C. I. Lewis would be of no avail for the interpretation of the strong "if... then..." as here understood, for it refers to a purely logical relationship of entailment, whereas the concept under consideration will, in general, represent a nomological relationship, i. e., one based on empirical laws. For recent discussions of the problems of counterfactuals and laws, see Langford (18); Lewis (20), pp. 210-230; Chisholm (10); Goodman (14); Reichenbach (26), Chapter VIII; Hempel and Oppenheim (16), Part III; Popper (24).

[2] Cf. Carnap (6); a brief elementary exposition of the central idea may be found in Carnap (7), Part III. The partial definition (R) formulated above for the expression "$T(x)= c$" illustrates only the simplest type of reduction sentence, the so-called bilateral reduction sentence.

of "temperature" may then be gradually extended to cases not covered in (R) by laying down further reduction sentences, which reflect the measurement of temperature by devices other than thermometers.

Reduction sentences thus provide a means for the precise formulation of what is commonly referred to as operational definitions [1]. At the same time, they show that the latter are not definitions in the strict sense of the word, but rather partial specifications of meaning.

The preceding considerations suggest that in our characterization (3.2) of empiricist languages we broaden the provision *a* (3) by permitting in the vocabulary of L all those terms whose meaning can be specified in terms of the basic empirical vocabulary by means of definitions or reduction sentences. Languages satisfying this more inclusive criterion will be referred to as thing-languages in the wider sense.

If the concept of empiricist language is broadened in this manner, then the translatability criterion (3.1) covers—as it should—also all those statements whose constituent empirical terms include "empirical constructs", i.e., terms which do not designate observables, but which can be introduced by reduction sentences on the basis of observation predicates.

Even in this generalized version, however, our criterion of cognitive meaning may not do justice to advanced scientific theories, which are formulated in terms of "theoretical constructs", such as the terms "absolute temperature", "gravitational potential", "electric field", "ψ function", etc. There are reasons to think that neither definitions nor reduction sentences are adequate to introduce these terms on the basis of observation predicates. Thus, e.g., if a system of reduction sentences for the concept of electric field were available, then —to oversimplify the point a little—it would be possible to describe, in terms of observable characteristics, some necessary and some sufficient conditions for the presence, in a given region, of an electric field of any mathematical description, however complex. Actually, however, such criteria can at best be given only for some sufficiently simple kinds of fields.

[1] On the concept of operational definition, which was developed by Bridgman, see, for example, Bridgman (3, 4) and Feigl (12).

Now theories of the advanced type here referred to may be considered as hypothetico-deductive systems in which all statements are logical consequences of a set of fundamental assumptions. Fundamental as well as derived statements in such a system are formulated either in terms of certain theoretical constructs which are not defined within the system and thus play the rôle of primitives, or in terms of expressions defined by means of the latter. Thus, in their logical structure such systems equal the axiomatized uninterpreted systems studied in mathematics and logic. They acquire applicability to empirical subject matter, and thus the status of theories of empirical science, by virtue of an empirical interpretation. The latter is effected by a translation of some of the sentences of the theory—often derived rather than fundamental ones—into an empiricist language, which may contain both observation predicates and empirical constructs. And since the sentences which are thus given empirical meaning are logical consequences of the fundamental hypotheses of the theory, that translation effects, indirectly, a partial interpretation of the latter and of the constructs in terms of which they are formulated.[1]

In order to make translatability into an empiricist language an adequate criterion of cognitive import, we broaden therefore the concept of empiricist language so as to include thing-languages in the narrower and in the wider sense as well as all interpreted theoretical systems of the kind just referred to.[2] With this understanding, (3.1) may finally serve as a general criterion of cognitive meaning.

[1] The distinction between a formal deductive system and the empirical theory resulting from it by an interpretation has been elaborated in detail by Reichenbach in his penetrating studies of the relations between pure and physical geometry; cf., e. g., Reichenbach (25). The method by means of which a formal system is given empirical content is characterized by Reichenbach as "coordinating definition" of the primitives in the theory by means of specific empirical concepts. As is suggested by our discussion of reduction and the interpretation of theorical constructs, however, the process in question may have to be construed as a partial interpretation of the non-logical terms of the system rather than as a complete definition of the latter in terms of the concepts of a thing-language.

[2] These systems have not been characterized here as fully and as precisely as would be desirable. Indeed, the exact character of the

5. *On "the meaning" of an empirical statement*

In effect, the criterion thus arrived at qualifies a sentence as cognitively meaningful if its non-logical constituents refer, directly or in certain specified indirect ways, to observables. But it does not make any pronouncement on what "the meaning" of a cognitively significant sentence is, and in particular it neither says nor implies that that meaning can be exhaustively characterized by what the totality of possible tests would reveal in terms of observable phenomena. Indeed, *the content of a statement with empirical import cannot, in general, be exhaustively expressed by means of any class of observation sentences.*

For consider first, among the statements permitted by our criterion, any purely existential hypothesis or any statement involving mixed quantification. As was pointed out earlier, under (2.2)(*a*), statements of these kinds entail no observation sentences whatever; hence their content cannot be expressed by means of a class of observation sentences.

And secondly, even most statements of purely universal form (such as "All flamingoes are pink") entail observation sentences (such as "That thing is pink") only when combined with suitable other observation sentences (such as "That thing is a flamingo").

This last remark can be generalized: The use of empirical hypotheses for the prediction of observable phenomena requires, in practically all cases, the use of subsidiary empirical hypotheses [1]. Thus, e.g., the hypothesis that the agent of tuberculosis is rod-shaped does not by itself entail the consequence that upon looking at a tubercular sputum specimen through a microscope, rod-like shapes will be observed: a large number of subsidiary hypotheses, including the theory

empirical interpretation of theoretical constructs and of the theories in which they function is in need of further investigation. Some problems which arise in this connection—such as whether, or in what sense, theoretical constructs may be said to denote—are obviously also of considerable epistemological interest. Some suggestions as to the interpretation of theoretical constructs may be found in Carnap (8), section 24, and in Kaplan (17); for an excellent discussion of the epistemological aspects of the problem, see Feigl (13).

[1] This point is clearly taken into consideration in Ayer's criteria of cognitive significance, which were discussed in section 2.

of the microscope, have to be used as additional premises in deducing that prediction.

Hence, what is sweepingly referred to as "the (cognitive) meaning" of a given scientific hypothesis cannot be adequately characterized in terms of potential observational evidence alone, nor can it be specified for the hypothesis taken in isolation: In order to understand "the meaning" of a hypothesis within an empiricist language, we have to know not merely what observation sentences it entails alone or in conjunction with subsidiary hypotheses, but also what other, non-observational, empirical sentences are entailed by it, what sentences in the given language would confirm or disconfirm it, and for what other hypotheses the given one would be confirmatory or disconfirmatory. In other words, the cognitive meaning of a statement in an empiricist language is reflected in the totality of its logical relationships to all other statements in that language and not to the observation sentences alone. In this sense, the statements of empirical science have a surplus meaning over and above what can be expressed in terms of relevant observation sentences.[1]

6. *The logical status of the empiricist criterion of meaning*

What kind of a sentence, it has often been asked, is the empiricist meaning criterion itself? Plainly it is not an empirical hypothesis; but it is not analytic or self-contradictory either; hence, when judged by its own standard, is it not devoid of cognitive meaning? In that case, what claim of soundness or validity could possibly be made for it?

One might think of construing the criterion as a definition which indicates what empiricists propose to understand by a cognitively significant sentence; thus understood, it would not have the character of an assertion and would be neither true nor false. But this conception would attribute to the criterion a measure of arbitrariness which cannot be reconciled with the heated controversies it has engendered and even less with the fact, repeatedly illustrated in the present article, that the changes in its specific content have always been deter-

[1] For a fuller discussion of the issues here involved cf. Feigl (13) and the comments on Feigl's position which will be published together with that article.

mined by the objective of making the criterion a more adequate index of cognitive import. And this very objective illuminates the character of the empiricist criterion of meaning: It is intended to provide a clarification and *explication* of the idea of a sentence which makes an intelligible assertion.[1] This idea is admittedly vague, and it is the task of philosophic explication to replace it by a more precise concept. In view of this difference of precision we cannot demand, of course, that the "new" concept, the explicatum, be strictly synonymous with the old one, the explicandum.[2] How, then, are we to judge the adequacy of a proposed explication, as expressed in some specific criterion of cognitive meaning?

First of all, there exists a large class of sentences which are rather generally recognized as making intelligible assertions, and another large class of which this is more or less generally denied. We shall have to demand of an adequate explication that it take into account these spheres of common usage; hence an explication which, let us say, denies cognitive import to descriptions of past events or to generalizations expressed in terms of observables has to be rejected as inadequate. As we have seen, this first requirement of adequacy has played an important rôle in the development of the empiricist meaning criterion.

But an adequate explication of the concept of cognitively significant statement must satisfy yet another, even more important, requirement: Together with the explication of certain other concepts, such as those of confirmation and of probability, it has to provide the framework for a general theoretical account of the structure and the foundations of scientific knowledge. Explication, as here understood, is not a mere description of the accepted usages of the terms under consider-

[1] In the preface to the second edition of his book, Ayer takes a very similar position: he holds that the testability criterion is a definition which, however, is not entirely arbitrary, because a sentence which did not satisfy the criterion "would not be capable of being understood in the sense in which either scientific hypotheses or common-sense statements are habitually understood" ((1), p. 16).

[2] Cf. Carnap's characterization of explication in his article (9), which examines in outline the explication of the concept of probability. The Frege-Russell definition of integers as classes of equivalent classes, and the semantical definition of truth—cf. Tarski (30)—are outstanding examples of explication. For a lucid discussion of various aspects of logical analysis see Pap (21), Chapter 17.

ation: it has to go beyond the limitations, ambiguities, and inconsistencies of common usage and has to show how we had better construe the meanings of those terms if we wish to arrive at a consistent and comprehensive theory of knowledge. This type of consideration, which has been largely influenced by a study of the structure of scientific theories, has prompted the more recent extensions of the empiricist meaning criterion. These extensions are designed to include in the realm of cognitive significance various types of sentences which might occur in advanced scientific theories, or which have to be admitted simply for the sake of systematic simplicity and uniformity, [1] but on whose cognitive significance or non-significance a study of what the term "intelligible assertion" means in everyday discourse could hardly shed any light at all.

As a consequence, the empiricist criterion of meaning, like the result of any other explication, represents a linguistic proposal which itself is neither true nor false, but for which adequacy is claimed in two respects: First in the sense that the explication provides a reasonably close *analysis* of the commonly accepted meaning of the explicandum—and this claim implies an empirical assertion; and secondly in the sense that the explication achieves a "*rational reconstruction*" of the explicandum, i.e., that it provides, together perhaps with other explications, a general conceptual framework which permits a consistent and precise restatement and theoretical systematization of the contexts in which the explicandum is used—and this claim implies at least an assertion of a logical character.

Though a proposal in form, the empiricist criterion of meaning is therefore far from being an arbitrary definition; it is subject to revision if a violation of the requirements of adequacy, or even a way of satisfying those requirements more fully, should be discovered. Indeed, it is to be hoped that

[1] Thus, e. g., our criterion qualifies as significant certain statements containing, say, thousands of existential or universal quantifiers—even though such sentences may never occur in every-day nor perhaps even in scientific discourse. For indeed, from a systematic point of view it would be arbitrary and unjustifiable to limit the class of significant statements to those containing no more than some fixed number of quantifiers. For further discussion of this point, cf. Carnap (6), sections 17, 24, 25.

before long some of the open problems encountered in the analysis of cognitive significance will be clarified and that then our last version of the empiricist meaning criterion will be replaced by another, more adequate one.

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The Mind-Body Problem in the Development of Logical Empiricism

by Herbert Feigl

The cluster of puzzles and perplexities that constitute the the Mind-Body-Problem of modern philosophy owes its origin to a great variety of motives and considerations. The central issue, however, may justly be located in the disputes between Dualism and Monism. The dualistic doctrines have a twofold root: Firstly, there are the mythological, animistic, theological and religious-moral contentions as to the sharp distinction, if not actual separability, of the mental and the physical. The deep-rooted and culturally fairly widespread wishful belief in some form of survival after bodily death, as well as the exaltation of the spirit and the deprecation of the flesh in so many eastern and western religions and moral codes may be regarded as the emotional root of dualism. The other, scientific, root of dualism may be found in the rise of science, most prominently beginning with the 17th century, although at least adumbrated in ancient thought. The striking success of the method of the physical sciences was, at least historically, contingent upon a clear cut division of the physical and the mental, and the relegation of the latter to the limbo of a sort of secondary or epiphenomenal existence. But the development of modern psycho-physics and psycho-physiology from the 19th century on, culminating in presentday neurophysiology, Gestalt-psychology, psycho-somatic medicine and cybernetics, has revived the interest in monistic interpretations. One discrepant tendency may of course be seen in the dualistic claims of the researchers in the still highly questionable fields of Parapsychology (extrasensory perception, psychokinesis,

interactionistic dualism. The last proviso indicates the systemic nature of the proposed identifications. On the whole, I should think, the available evidence points with remarkable consistency in the direction of a system of psychology, psychophysics and psychophysiology wich provides for the monistic solution here outlined. But this is the empirical, the factual issue which philosophical analysis cannot decide and should not prejudge. We can do no more than clarify the logical structure of the problem and remove unfounded objections to the identity theory which perhaps owing to a failure of nerve seems to have been temporarily eclipsed by a return to parallelism—if not even interactionism. The view we are proposing here should not be construed as a metaphysical doctrine. It again has merely some historical affinities with certain forms of epiphenomenalistic materialism, panpsychism, or the double-aspect or double-knowledge theories. If a label is wanted, then perhaps "double-language-theory" is still the least misleading I can suggest. Within the conceptual system which fulfills the intersubjective confirmability condition and is at the same time the simplest account compatible with the accumulated facts of psychology, the terms of the behavioral-psychological and of the introspective language are (systemically) synonymous. If further factual discoveries should force upon us a radical revision of the conceptual system, then, conceivably, this claim of synonymity may have to be modified or even abandoned. In the meantime it is well to remember that the tentative indentifications which generally underlie synonymities of this type are among the most fruitful devices in the search for unifying explanations in the progress of science. The identification of light with a special kind of electromagnetic oscillating field; ferro-magnetism with the spin of electrons; of heat with molecular motion; of chemical valencies with certain dynamical features of the atoms; of the medium of inheritable traits with the gene-structure of the chromosomes; etc., these are only some of the more noteworthy cases in point.

One last critical question requires discussion. The entire preceding argument, it may be urged, depends upon the presupposition that the vocabulary of introspection is part of an intersubjective language and thus really interpreted behavioristically. Introspective terms are then introduced on the evi-

identity. The meaning of a concept is determined, not by its pictorial connotations, but by the system of rules which implicitly defines that meaning. If two terms, no matter what words or symbols they are and no matter what pictorial appeals they may convey, are mutually substitutible for each other because they fulfill precisely the same functions in a system of rules, then they have the same meaning, they are the same concept.

The application of these considerations to the mind-body problem must by now be fairly obvious. Relative to the "molar" (or macro-) account given by behavioristic psychology, the neurophysiological account is a micro-description of the very same events and processes. The pictorial connotation of the two accounts are of course different, since the images attaching to the behavioristic terms represent stimulus-response situations, while the images connected with the neurophysiological language are apt to represent observations of nervous tissues. The notoriously greatest difficulty however arises here from the pictorial connotations of the mentalistic terms that owe their introduction to a third avenue of approach to the same processes —, *introspection*. The qualities of direct awareness, the facts of stimuli and responses, the directly observable data of the neurophysiologist are of course not to be identified with one another. We have already warned against the fallacies involved here. But we contend that the designata of the mentalistic language are identical with the descripta of the behavioristic language, and that both are identitical with the designata of the neurophysiological language. Utilizing the distinction suggested before, we may say that the factual reference of some of the terms in each of these different languages (or vocabularies) may be the same while only their evidential bases differ. A state of mind, conceived as an event in the spatio-temporal-causal structure of the world may thus be characterized by concepts that are evidentially anchored in quite heterogeneous areas. It is this anchoring that gives the concepts their particular place in one or the other vocabulary. But if we are sure not to confuse their factual reference with their evidential base we may rightly say that they have the same meaning. This holds unless we countenance in principle unconfirmable assertions or unless the facts of psychology themselves force upon us an

theory of electricity, magnetism, electrolysis and heat. Those doubts could of course never be removed with finality. We not only admit but would even emphasize the empirical or inductive basis which underlies all such identifications in the realm of factual knowledge. The only kind of identification that can be proved with finality is found in the purely formal sciences. Despite the fundamental difference between the situation in empirical knowledge and that in pure mathematics, there is an instructive structural analogy here. Two different infinite series, for example, may be used for the definition (unique description) of one and the same number, as e. g. in the case of π. But such mathematical proofs of identity also presuppose a frame of concepts and postulates. Only within such a frame can we assert meaningfully and demonstrate validly the identity of the object of two descriptions. (A perfectly obvious illustration is the arithmetical identity of 2^3 with $\sqrt{64}$). The frame of arithmetic, i. e. the postulate system of Peano in the Frege-Russell interpretation, is logically or analytically valid. The situation is radically different in empirical geometry. For example the identity of two points or line-segments characterized in different ways depends upon the factual adequacy of the geometrical postulates. The same holds, *a fortiori*, for the identifications in the natural sciences. Returning to an illustration previously introduced, the identification of the temperature of a gas with the mean kinetic energy of its molecules depends of course upon the truth of the molecular-statistical theory of heat. But if the truth of the theory is assumed, the strict identity of reference becomes a matter of logical deduction. Temperature as a macro-concept refers to the state of a body which is only more fully characterized by the theoretical description of its micro-structure. Once the theory is adopted it would make no sense to speak of the temperature as something distinct and different from that set of micro-conditions. Only the pictorial connotations of the word "temperature" that remind us of thermometers or of the directly felt heat of a body seem to make the corresponding concept merely "parallel" to that of molecular thermodynamics.

The logical principle that underlies our argument is, as indicated before, simply a variant of Leibniz' principle of

identity of concepts. What is the criterion for identity? We can safely follow Leibniz' *principium identatis indiscernibilium*, here as elsewhere. If two terms are defined by the same set of rules, they are merely different symbols for the same meaning, they are the same concept. Such synonymity however may arise in various ways. The most obvious and trivial case is that of explicit definition in which we arbitrarily stipulate the unrestricted mutual substitibility of symbols. More interesting and more relevant for our problem is the case of epistemic (or "systemic") synonymity. We may determine certain meanings uniquely by different definitions of the type known as "definite descriptions" (Russell). Thus two explorers may unwittingly have observed the same mountain from different directions, and only after comparing notes come to realize that it was really identically the same mountain. This is a systemic identity in that it can be established only if the system of empirical geometry and optics is presupposed. Quite analogously, the identity of the morning star with the evening star (ever since Frege a much used example in logical analyses), is based on the recognition that one and the same trunk of world-lines (the four-dimensional representation of the planet Venus) is the object of reference of the two designations, referring to alternative segments of that trunk. Only within the system of Kepler's kinematics and of ordinary geometrical optics can this identity be explicated and warranted. This and the preceding example concerned the identity of things (continuants), or more precisely speaking, the identity of the designation of a name with the designatum (descriptum) of a description or else the identity of the descripta of two descriptions of thinglike entities.[1] But quite similar considerations hold for concepts (predicates of various levels). The identity of the concept of "electric current" defined by various definite descriptions such as those based on the magnetic, chemical or thermal indications can be defended against empiricist or operationist doubts only after a full fledged system of electrodynamics enables us to *deduce* those various effects from a unitary

[1] Even these first two examples, could be analyzed in terms of individual-concepts (unit-classes) instead of things, whose identity is under examination.

human history and unendowed with human sentiments that these events "mean" a commemoration of the armistice. This fascinating argument however, rests on two fallacies. Firstly, such a utopian knowledge of the neurophysiological processes would enable the Martian to derive the actual and potential verbal behavior of the Londoners; it would also enable him to reconstruct the physical account of the origin of the ritual (two minutes silence, etc.) and thus to know, in principle, everything that can be known about those events in an intersubjective manner. Secondly, this can be achieved even if the Martian, because of the differences or limitations in his repertoire of emotions, cannot empathize, let alone share, the sentiments in question. A congenitally blind man, equipped with modern physical devices, could investigate not only the physics of colored surfaces, of light radiations reflected by them, etc., but also the (behavioristic) psychology of color sensation, discrimination and perception (on the part of subjects equipped with eyesight). Similarly, a Martian could *know* all *about* human feelings and emotions without having *knowledge of* them, i. e. without directly experiencing them or being *acquainted* wiht them by intuition or imagination.

Quite generally, one of the difficulties that are so frequently adduced in the critique of the identity-theory of mind and body rests on a confusion of *acquaintance* with *knowledge*. No one denies that the *image* of a brain, as perceived by a surgeon or as pictured in terms of an atomic model has totally different properties from a melody-as-heard or a sentiment-of-elation-as-actually-lived-through. But images or other directly experienced acts or data are not in and by themselves concepts. Knowledge proper is always conceptual. This insight is an important point of agreement between such otherwise divergent recent philosophers as Poincaré, Bergson, James, Dewey, Russell, Eddington, R. W. Sellars, C. I. Lewis, Schlick, Wittgenstein and Carnap. What then is meant by "conceptual knowledge"? What is meant by "concept"? The best answer we can give today rests on a repudiation of psychologism and upon the results of pure semiotic. Concepts are symbols whose meaning is constituted by the syntactical, semantical and pragmatic rules which determine the relations of those symbols to one another, to their designata and to their evidential basis. The crucial question then concerns the conditions of the

quite generally of the negativism of orthodox positivism then all the relationships mentioned are not identities, but—at best—lawful (causal) connections between distinguishable states or events. The equivalence of statements about each pair of states or events can therefore be only of the empirical type. The precipitous assertion of a logical equivalence was of course based on the phenomenalistic claims of the explicit definability of the entities in one realm in terms of the entities of the corresponding other realm. This, as we have tried to point out, was completely unwarranted.

Curiously enough, the same sort of critique has been applied also to the identification of mental states with processes inside the organism, i. e. neurophysiological processes. It seemed quite incredible how a color sensation, a remembrance of things past, an act of thought concerning mathematical relations, or a feeling of indignation, could in any sense whatsoever be "the same" as some brain-process or other. Here again it was urged that the relation can be no other than, at best, that of a lawful correspondence or parallelism of simultaneous events. The many arguments in favor of this view are well known. One of the more important among these arguments contends that the attributes of mental states and events and the attributes of the corresponding neurophysiological processes are so different that the respective predicates characterizing each of the two types of processes can stand only in the relation of general (empirical) equivalence but never in that of a logical equivalence. Hume argued that statements of specific causal relations are synthetic *a posteriori* because alternatives are always conceivable without self-contradiction. Similarly, it is contended that a brain process which a future neurophysiology might characterize as of a definite type could conceivably be associated with a phenomenologically described immediate experience of a type radically different from that with which, as a matter of empirical regularity, it is actually associated (say, a sentiment of nostalgia). Eddington once argued that even the most detailed physiological and physical knowledge of the behavior and the nervous processes in the human organisms occurring on some November 11th at 11 a. m. in London could not possibly indicate to a Martian super-scientist unfamiliar with

of specific unobserved or unobservable states of affairs would be impossible. Just which network of laws and existential assumptions will most adequately and parsimoniously serve for a comprehensive and predictively fruitful organization of the data can of course not be settled in any a priori fashion. Nevertheless, only within the frame of a language that makes such a network possible can we legitimately assign probabilities to hypotheses on the basis of relevant evidence. The ("realistic") frame itself however cannot be justified by considerations of inductive probability. The adoption of this frame can be vindicated only by its fruitfulness for the purposes which it helps to fulfill. Like other principles which rationalists mistake for synthetic *a priori* presuppositions this is, from the viewpoint of logical reconstruction, a basic convention, capable only of pragmatic but not of cognitive justification.[1]

(3) We are now ready to develop the implications of the just outlined clarified empirical realism for the mind-body problem. There are three demonstrably mistaken reductions by means of which monistic solutions have been attempted. There is firstly the crude and simple-minded identification of the stimulus-aspects with the mental qualities. Obviously we cannot say that a color sensation is identical with the radication (of a certain intensity and frequency-pattern) which, under certain conditions merely elicits that sensation. Secondly, in our critique of phenomenalism we have also refuted the identification of physical bodies with complexes or configurations of elements of direct perception. Thirdly, the behavioristic identification of mental states with the responses (including linguistic utterances) of organisms is equally fallacious. It is of course granted that the confirmation of objective statements concerning "physical" bodies is possible only on the basis of the evidence of direct experience. Similarly, intersubjectively meaningful statements concerning mental states are confirmable only on the basis of behavioral evidence. If we are to avoid the errors of phenomenalist reduction and

[1] The realism of pure semantics and pragmatics is outlined in (134, 194, 195) and an analysis of the problem of justification may be found in (133).

of practically all scientific statements we implicitly allow for a genuinely critical realism. This new version of realism is free from the objectionable metaphysical elements in the older forms of realism. Much of the perplexities in the time-honored reality-problems arose out of confusion of the intuitive, experiential idea of reality with the cognitive, objective concept of reality. The agonies that attend all attempts to solve the "problem of transcendence" can be avoided once it is realized that this is a pseudo-problem. The solution that had been sought involved plainly an inconsistency: The non-given was to be proved just as real as the given. But if by "real" one means *given*, then obviously the wish for a demonstration is doomed because of the self-contradiction. If however one wishes to connect with the word "real" not an ineffable but a cognitively expressible significance then the usage of this term in common life and in science may profitably be taken as a standard. "Real" and "unreal" are of course ambiguous and often emotively tinged words. But in the context of the traditional realism-phenomenalism controversy it is clear that the distinction connoted by these terms cannot be intended to achieve a division among things, events or processes. Once anything is at all classified under one of these three headings it is *eo ipso* considered real. Dreams and delusions are (even according to commonsense) real enough as occurrent events. What is not real are the referents (designata) of certain terms or assertions that we sometimes formulate on the basis of certain *interpretations* of dream or delusion-experiences.

The realistic correction of positivism consists in the identification of meaning with factual reference. This conforms well with customary usage according to which a statement *means* a state of affairs; and is *true* if that state of affairs is fulfilled ("is real", "exists"). This is the obvious grammar of "meaning", "truth" and "reality". Metaphysical problems cannot arise as long as we combine those definitions with the empiricist requirement that in order to be meaning*ful*, a statement must in principle be confirmable. The confirmation rules which formulate the connections between the evidential basis and the factual referents of statements are the metalinguistic correlate of those laws without which inference

reflexes, verbal responses, etc., but must not be confused with the emotion itself. Generally, the "theoretical constructs" of the sciences cannot be identified with (i. e. explicitly defined in terms of) concepts which apply to the directly perceptible facts as they are manifest in the contexts of ordinary observation or of experimental operations.

(2) The required correction and emendation of the phenomenalistic phase of positivism and operationism can best be achieved by means of a reconstruction in terms of pure semantics and pure pragmatics. Semantics as developed primarily by Tarski and Carnap enables us in a precise way to speak, in a metalanguage, about the relation of designation that holds between the symbols of a given language (the object language) and the objects, properties, relations and states of affairs they symbolize. The required metalanguage must of course have a sufficiently rich vocabulary to allow for this. It is in the field of pure pragmatics (thus far only sketched in outline by Wilfrid Sellars) that the rules and the scope of the metalanguage are determined. The pragmatic prerequisites of a workable scientific language extend far beyond the conditions that must be fulfilled for the sake of logical consistency and for the purposes of deductive inference. They also include the condition of confirmability, with all that this implies: a set of proper names (or co-ordinates) and of predicates only some of which correspond to directly confrontable items of immediate experience; a set of relationships that connect the directly verifiable with the only (indirectly) confirmable predicates and statements. With such a reconstruction a distinction necessarily neglected by phenomenalism can be reinstated. It is the important distinction between the evidential basis and the factual reference of terms and statements. In acknowledging this distinction we retain the empiricist conditions for meaningfulness and for factual adequacy: Only if our terms are nomologically related to terms that designate items or aspects of what is directly observable can they be factually meaningful; and only if statements are supported at least by incomplete and/or indirect evidence can they be justifiably asserted. But in the recognition of the incompleteness and indirectness of the verification

positivism and radical operationism (and behaviorism); (2) a re-instatement of a clarified critical realism on the basis of pure semantics and pure pragmatics; (3) a return to a reinterpreted identity (or double-language) view of mind and body.

(1) The slogan of Vienna Logical Positivism: "The meaning of a statement is the method of its verification"; (190) and the slogan of Bridgman's operationism (5): "A concept is synonymous with the set of operations" [which determine its applications] were excellent preventives of the transcendent type of metaphysical speculations. They have had a most salutary purifying effect. Logical empiricism in its later development, however, had to replace these radical principles by more conservative ones. Als already indicated, the meaning of scientific statements cannot in general be identified with their confirming evidence. This is obvious in all those cases in which the evidence must in principle be indirect. Historical statements concerning past events, predictions of future events; existential hypotheses concerning radiations, subatomic processes in physics; genes, filterpassing viruses in biology; unconscious motivations in psychology; etc., are only some of the more striking types of assertions whose meanings (i. e. the states of affairs to which they refer) cannot be identified with the states of affairs that can conceivably serve as evidence for them. For a more specific but very simple example we may refer to the concept of the temperature of a body. As ordinary and scientific common-sense (untouched by ultra-positivistic reductionism) would put it, thermometer (or pyrometer) readings, spectroscopic findings, and other types of measurement merely indicate something about the body in question, namely the intensity of heat which is a state of that body. No matter whether this heat intensity is construed in terms of classical (macro-) thermodynamics or in terms of statistical (micro-or molecular) thermodynamics, it is in any case only *evidenced by but not identical with* those indications. Similarly for psychology: The overt symptoms and behavior that indicate an emotion, like e. g., anxiety, are confirmable and measurable in terms of skin temperature, endocrine secretions, psychogalvanic

Felix Kaufmann (34), and similarly also Norman Jacobs (150), generally in sympathy with the principles of Logical Empiricism, insisted that strict identity would have to be tantamount to *logical* equivalence of phenomenal (introspective) descriptions of mental states with the descriptions of the "correlated" neurophysiological processes. But it seems obvious, so Kaufmann argued essentially, that the investigations of psycho-physiology are of a factual-empirical character. Which mental state is correlated with which neural processes can be determined only by experimental investigations. The statement of the correlation is therefore synthetic and the "equivalence" of the two descriptions thus can at best be only of (universal) *empirical* character. Reading this conclusion again in terms of traditional metaphysics it may be taken as a formulation of dualistic parallelism. Wolfgang Köhler in one of his later works,[1] and other thinkers trying to be cautious in such delicate matters, have essentially retreated to this obviously safer (because less daring) position. If anyone (like, e. g., E. G. Boring[2]) wanted to account for the parallelism by means of a supposed more fundamental identity, he usually availed himself of the principle of parsimony.

The principle of parsimony itself needs careful analysis. Occam's razor has really, as it were, *three* blades. The simplicity it advocates may be the descriptive or purely formal (or logico-mathematical) expediency that distinguishes, e. g., the heliocentric from the geocentric description of the planetary system. It may be the factual (or inductive) simplicity that arises from a reduction of the number of independent empirical hypotheses. This is probably the purport of Newton's *regula philosophandi*. But finally, Occam's razor may be used to cut away metaphysical entities. In what follows I shall contend that this third blade, the confirmability criterion of Carnap and Reichenbach, if properly applied, removes the metaphysical surplus, without cutting into the flesh of knowledge. I shall contend also that this new point of view involves (1) a fundamental revision of phenomenalistic

[1] *The Place of Values in a World of Facts*, Liveright, New York, 1936.
[2] *The Physical Dimensions of Consciousness*, Appleton, New York, 1933.

development of physicalism. The first phase was rather rash in its claim of the translatability of the statements of physics and those of psychology into those of the thing-language. Availing ourselves of the material idiom (realistic language), this radical and crude form of physicalism may be said to amount to an identification of mental states with overt behavior. Early behaviorism (especially that of J. B. Watson) has been rightly accused of just this fallacious reduction. This view was essentially revised and corrected in the later formulations (119, 119*a*). Strict translatability depends of course on explicit definitions. But no explicit definitions that would serve the purpose could plausibly be constructed. The concepts of physics and psychology could perhaps be *introduced* by means of test-condition-test result conditionals but not in any way be regarded as synonymous with concepts of the thing-language (or purely logical compounds thereof). Carnap (119) advanced his reduction sentences as a possible formulation of those conditionals. While it has become increasingly doubtful that this formulation is logically adequate, the underlying and related ideas of confirmability and degree of confirmation are now quite generally accepted. No statement of physics nor of (intersubjective) psychology can be considered as completely and directly verifiable (or refutable) by the observations as formulated in the protocol-statements of the thing-language. The protocol-statements confer only a degree of confirmation upon the statements in the scientific languages of physics and psychology.

Reichenbach's version of scientific empiricism (64, 176) had for many years opposed the narrow verifiability criterion of the Viennese positivists. His emphasis on probability and induction, led him to advocate a more inclusive confirmability criterion, amounting approximately to the same delimitation of factual meaning as Carnap's criterion (in the second phase of physicalism). Reichenbach's account of the mind-body problem, based on his empirical realism, represents in many ways a position similar to that of Schlick in his early realistic approach. Before we turn to a fuller discussion of this view we must briefly mention a more agnostic position which arose out of a reaction against the earlier, rather immature arguments in favor of mind-body identity.

on a phenomenalistic basis. It is therefore not surprising that metaphysicians misinterpreted this approach as a revival of a Berkeleyan subjective idealism. While Carnap explicitly disavowed any claims regarding the ultimate reality-problems of the mental and the physical, he shared of course with Berkeley, Hume, Condillac, Mill, Mach and Avenarius the conviction that there is no ontological mind-body problem that could be legitimately formulated. The only genuine problem, Carnap claimed, was one of logical analysis, i. e. the question of the formal relations between the concepts that describe the data of first-person-experience, the concepts of physics and those of (behavioristic) psychology. The "basic situation" of the mind-body-relation was identified with the parallelism of data that a person would experience if he were to observe by means of some "cerebroscope", his own cerebral processes alongside with the stream of images or feelings which "correspond" to those brain processes. But the internal difficulties of a strictly phenomenalistic reconstruction were soon recognized. The translatability of statements concerning physical objects into statements concerning phenomenal data could no longer be held to obtain in the sense of mutual deducibility. And the absurdities of a metaphysical solipsism were parallelled by the absurdities of a phenomenal language that was doomed to be "private", "soliloquistic", "incommunicable".

The second phase of logical positivism arose largely out of a reaction against the phenomenalism (experientialism) of the first phase. Under the influence of O. Neurath's and K. Popper's critical suggestions, Carnap (10, 116, 118, 119, 119*a*) formulated his *physicalism*. It was easy again for metaphysically minded opponents to misconstrue this position as a variant of ontological materialism. But Carnap's aim was, just as in the previous phase, merely that of an analysis of language. He outlined a logical reconstruction of factual knowledge on the basis of an intersubjective (physicalistic) thing-language. This position, though independently arrived at, was generally akin to the methodological behaviorism that had been formulated even somewhat earlier but with much less formal precision by E. A. Singer (90) and K. S. Lashley (155). It is important to distinguish two phases in the

monistic position, however reformulated in a more cautious and therefore more auspicious manner. In connection with the very brief review of the four previous positions that I am now going to present, it must be kept in mind that the affinities these positions display with the more traditional metaphysical doctrines are, on the wole, more of the nature of historical analogies than genuine identities of theoretical import. Logical empiricists have from the beginning disclaimed any intention of pronouncing ontological truths. Their sole concern has been the analysis of language and meaning. It was precisely on the basis of such reflections that ontologies of *all* sorts were declared as devoid of factual meaning. The metaphysicians, understandably hurt in their pride and unconvinced by the negativism of the positivists, kept reading into the logical analysis of the latter all the traditional tenets and categories. As already admitted, the flavor of the traditional monisms (or of parallelism) was there, but only historically-culturally speaking. The first position, for example, can easily be regarded as a double-aspect, or double knowledge view of the type held by critical realism. This was Schlick's (84) outlook before the formation of the Vienna Circle, i. e. before the impact of the ideas of Carnap and Wittgenstein.[1] However, even anticipating the later emphases of logical positivism, Schlick regarded the difference of the mental and the physical as a difference between two conceptual systems, of which the physical, as a matter of fundamental empirical fact, is universally applicable whereas the psychological pertains only to a small part of the total realm of reality. This early point of view is therefore more appropriately characterized as a "double-language" theory.

With the first phase of logical positivism, most markedly represented by Carnap's "*Der Logische Aufbau der Welt*", the rational reconstruction of empirical knowledge was pursued

[1] This widely held position may be traced back to Spinoza, and is represented in various metaphysical versions also by Leibniz, (in a certain sense also by Kant), Schopenhauer, Fechner, Clifford, Riehl, Paulsen, the American monistic critical realists, especially R. W. Sellars, D. Drake, C. A. Strong; by one phase of B. Russell's thought; by R. Ruyer in France; by the psychologists Ebbinghaus, M. Prince, Warren; the Gestalt psychologists, especially Köhler and Koffka; by L. T. Troland, E. G. Boring, C. K. Ogden, and others.

arose out of the considerations of "purpose", "free-choice", "reason" on the mental side as juxtaposed with "mechanism", "determinism", "cause" on the physical side. Normative and critical predications (like "corect", and "incorrect", "success" and "failure", responsible" and "irresponsible", "justified" and "unjustified", (morally) "right" and "wrong", etc., seem to apply meaningfully only to minds, mental states, attitudes or functions but not to physical things, processes or events.

This list of juxtapositions, which could easily be expanded, may serve as a reminder that any present-day-advocate of monism (in the sense of an identity-theory) is confronted with a considerable task. Recent naturalistic philosophical and psychological movements, such as positivism, pragmatism, neo-realism, behaviorism and some phases of analytic philosophy, have in one way or another attempted various resolutions of the puzzles posed by the apparent incompatibilities of the essential features of the mental and the physical. A good many of the traditional questions in the total complex of the problem have fairly generally been recognized as pseudo-problems, arising out of conceptual confusions. This may be asserted with assurance in the case of the free choice vs. determinism perplexity. Almost equally definite seem to me the clarifications of the problems of spatial localization, of emergent novelty and of teleology. The proper view of the referential, normative and critical functions of "mind" or "reason" depends on an adequate formulation of rule-guided behavior. Although a good deal of work along these lines is still required, it is evident even now that some of these questions pertain not so much to the distinction of the mental and the physical, but rather to that of logical structure to psychological (or behavioral) fact. Common to all these issues however is the irrepressible and most controversial question: In which sense is the identification of the mental and the physical to be understood? It is interesting to note that Logical Empiricism in the 25 years of its career since its beginnings in the Vienna Circle has in succession embraced three different monistic views and has temporarily countenanced also a more agnostic (parallelistic) form of dualism. In recent years Logical Empiricists have prepared a return to their first

etc.). Another and very different kind opposition comes from philosophers of various schools who either on the basis of their metaphysical commitments or simply in the name of clear thinking insist that the physical and the mental are *toto genere* and irreconcilably distinct and different so that any monistic attempts at their identification must be rejected on purely logical grounds.

This is not the place to review even in outline the history of dualistic and monistic arguments and systems from Descartes and Spinoza down to our time. Two notable conclusions seem to emerge from a study of this history:

(1) The clarification of the badly tangled issues requires as an indispensable first step the discrimination between the factual and the logical questions involved in the mind-body-problem. The factual questions depend for their solution on the progress of scientific research, such as in psycho-physiology. The philosopher qua logical analyst has no business either imaginatively to anticipate or dogmatically to endorse hypotheses that can be established only by painstaking empirical investigations. Since the philosopher is concerned with the analysis of meanings, he can at best examine the consistency of various hypotheses and clarify their precise content by an examination of their logical implications.

(2) It is evident that different thinkers have been impressed with different aspects of the very complex problem of the relations of the mental and the physical. Descartes was puzzled with the question how something of the nature of a non-spatial substance (thinking) could be causally related with a spatial substance (matter). Some philosophers of the 19th and 20th century tried to tackle another "spatial" problem: the location of sense data. Still others have tried to account for the difference of the mental and the physical in terms of the distinctions of the qualitative and the quantitative or of content and structure. Some were intrigued with the "private" character of consciousness and the "public" character of behavior and of neurophysiological processes. Others again, found in the "meaningful", "intentional", "referential", nature of mental states an insuperable obstacle in the attempted identification with "blind" brain-states. Similar objections

dential basis of linguistic responses and are therefore in any case logically on a par with those terms that have their basis in non-linguistic responses of the organisms. Thus, it may be said, that the real difficulty of the mind-body problem has been avoided rather than resolved. This objection obviously implies that the language of introspection is to be taken as phenomenal, purely experiental and thus strictly subjective. My reply, very briefly, is this: The problem thus proposed is the epistemological question of the relation between the "private" (if not solipsistic) language of data (phenomena) to the language of "public", intersubjective "constructs" (thing-concepts). It is highly questionable as to whether the idea of a phenomenal language in this sense can even be consistently maintained, let alone fully elaborated. But to those who cling to this "Aufbau" phase of positivism I would offer the suggestion that there can be only a correspondence, but never a translation between the phenomenal language and the thing language. If introspective descriptions are not to be taken as referring to events which are at least in principle confirmable by the much more indirect route of behavioral (or physiological) evidence, then they are indeed severed from the language of intersubjective communication and doomed to solipsistic privacy. There is no bridge between such a private language and the language of science except one of isomorphic correspondence. Structurally the situation bears a certain resemblance to the one in the reconstruction of the rational numbers on the basis of the natural numbers. Certain ordered pairs of natural numbers are introduced, they define rational numbers. But the rational numbers (like 3/1, for example) which represent integers (3 in this example) merely correspond to them, but are not identical with them. This isomorphism here consists not only in the one-to-one correspondence of certain elements of one realm to all elements in another, but in the one-to-one correspondence of the results of all arithmetical operations with corresponding elements. The analogy with the (however much more complex) field of epistemology lies in the isomorphism between certain statements in the phenomenal language and those in the intersubjective scientific language. As Carnap pointed out long ago (116) epistemological reconstruction may be attempted in either of two ways. The protocol-propositions may be part

of the system of the scientific language or they are outside of it. In the latter case we must have some statements in the scientific language that correspond to the protocol propositions. This correspondence, however, must not be confused with what is traditionally called psycho-physical or psycho-physiological parallelism. Parallelism has always been a doctrine according to which two different types of processes or two aspects of one and the same process are related by laws of coexistence or contemporaneity. The correspondence of the protocol propositions with propositions of the intersubjective system is a purely formal relation which arises exclusively out of the constructive definitions, involving differences in Russellian type-levels, by means of which the terms of the physical language are supposedly constituted out of terms belonging to the language of data. This is the position a consistent phenomenalist must take. But the many difficulties of that position have impelled Carnap and other physicalists to replace it by the reconstruction on an intersubjective basis. The analogy of this procedure in mathematics is of course the axiomatic method by means of which the total system of numbers (real numbers) is introduced and no problems of the "Aufbau"—type are then encountered. If the protocol propositions, i.e. the names and predicates occurring in them are part of the total symbolic system of the language of science, then we have here before us the sort of "realistic" reconstruction which underlies the systemic identity view of mind and body.

Résumé: Logical Empiricism in its present phase possesses the logical tools for a reformulation of the identity or double-language view of the mental and the physical. As in so many other issues of philosophy, this solution represents an equilibrium that has been reached only after several oscillations toward untenable extreme positions. The identity proposed is neither the reductive definitional one of phenomenalism or of behaviorism, nor is it an identity that presupposes a metaphysical realism. It is rather the hypothetical identity of the referents of terms whose evidential bases are respectively: introspective, behavioral or physiological. It is granted that the relations between the evidential indicators (linguistic responses, overt behavior and the data of neuro-physiology)

must be interpreted as empirical laws. But this does not in the least preclude the identity of the factual reference of the concepts which characterize the causal processes and events in terms of which the facts in each sphere of evidence may become explainable and predictable to an ever increasing extent. It is this hypothetical, systemic, referential identity that has been overlooked by those who retreated to a timid parallelism. The alleged difficulties of the identity view are mainly due to a confusion of pictorial appeals with cognitive meanings. A more adequate discussion of the points touched upon as well as of the many related questions and difficulties would of course require much more space than is available here. Many of the books and articles in the attached bibliography may help the reader to round out what has been merely sketched in the present essay.

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L'empirisme logique

par Marcel Barzin

I

Cet article veut être une critique du néo-positivisme. Mais je ne voudrais pas l'entreprendre sans rendre à cette doctrine un très sincère et très profond tribut d'admiration. Je me sens très proche des penseurs qui l'ont élaborée, et je leur dois sans doute beaucoup. Avant de marquer le point où je me sépare d'eux, je voudrais brièvement retracer les services qu'ils ont rendus à la philosophie de notre époque.

Tout d'abord, ils ont ramené la théorie de la connaissance à la théorie du langage scientifique. Ce qu'ils ont fait gagner en positivité à l'épistémologie par cette seule démarche est énorme. Tous les problèmes ont pris dès lors forme précise, et maints d'entre eux s'en sont trouvés résolus. Les amateurs de grandiloquence verbale — il y en a beaucoup, hélas! parmi les hommes qui s'occupent de philosophie — regretteront sans doute les obscurités des disputes d'autrefois. Mais tous ceux qui aiment la pensée claire et rigoureuse leur seront pour toujours reconnaissants de cette simplification décisive. D'autant plus qu'elle ne signifiait nul réel appauvrissement de la spéculation. Il y a peut-être, il y a probablement dans les ombres de l'inconscient une pensée sans images, et non verbale. Mais l'épistémologiste n'a pas à s'en occuper. Il doit se tourner vers les produits les plus achevés de la pensée humaine : ceux-là sont toujours exprimés en langage.

Substituer l'étude du langage à celle de la pensée avait pour résultat de séparer nettement les territoires de la philosophie et des sciences. Introduire la philosophie dans les sciences y crée des logomachies sans fin, qui ne sont point utiles, qui sont souvent nuisibles à la fécondité de la science.

Le philosophe, s'il n'est pas exposé aux mêmes dangers que le savant, n'a rien à gagner non plus à truffer son activité de notions empruntées aux sciences et qui, séparées de leur contexte naturel, s'altèrent et sont des sources de pseudo-problèmes.

De cette nouvelle définition, sortait une nouvelle théorie partagée en syntaxe qui étudiait la structure générale du langage et en sémantique qui étudiait le rapport du langage à ses objets.

La syntaxe a eu le mérite de populariser les notions de vérité formelle et de théorie déductive. Je dis populariser et non créer, car la notion de vérité formelle était déjà chez Frege, et l'école polonaise de logique l'y avait reprise; et la notion de théorie déductive avait été élaborée par les mathématiciens construisant leur axiomatique. Mais c'est l'école de Vienne qui a mis ces conceptions en pleine lumière et les a fait pénétrer dans la spéculation philosophique.

La vérité formelle, reposant sur des définitions, et devant sa valeur à une combinatoire simple, renvoyait dos à dos rationalistes et empiristes. Ces vérités cessaient, une fois leur nature bien comprise, d'être des à-priori mystérieux. Il ne pouvait plus être question de les considérer comme des données de l'expérience.

La théorie du système déductif, à prémisses purement arbitraires, jetait aussi des lumières nouvelles sur maint vieux problème. Il apportait partout un sain relativisme, dont découlait une sage règle de pensée. Avant de discuter quelque théorie, il fallait s'informer des prémisses sur laquelle elle reposait. Ce travail préliminaire fait presque toujours s'évanouir la discussion.

Il est moins facile de ramasser en quelques lignes les acquisitions de la sémantique. Cette partie du néo-positivisme est encore en pleine évolution. Mais l'abondance et la densité des travaux qu'on y consacre à l'heure actuelle montre une fois de plus qu'il s'est ouvert là un champ fertile de recherches positives.

Je mentionnerai seulement les remarquables travaux que l'école de Vienne a provoqués dans le champ du calcul des probabilités et de la théorie de l'induction. L'axiomatisation du calcul des probabilités par M. Reichenbach, la théorie de l'in-

duction de M. K. Popper sont des œuvres originales et de haute valeur.

Dans tous les domaines de l'épistémologie, l'école de Vienne a introduit une positivité et une sobriété de pensée qui ont renouvelé l'atmosphère de la recherche.

II

Pour l'œuvre positive qu'a construite le cercle de Vienne, on ne peut guère avoir que des éloges. C'est à ce qu'ils n'ont pas fait, et peut-être même à ce qu'il ont pensé qu'il ne fallait pas faire, que s'attacheront mes critiques. Ils ont élaboré une théorie du savoir scientifique, et ils ont cru épuiser ainsi tout le champ de la philosophie. C'est par cette attitude qu'ils se rapprochent indiscutablement des positivistes du siècle dernier.

La connaissance scientifique peut-elle résoudre tous les problèmes qui se posent à l'homme? Il en serait sans doute ainsi si l'homme était un esprit connaissant, et rien que cela. Mais l'homme n'est pas que cela. L'homme est aussi un être agissant. Et s'il agit, c'est qu'il pense que l'état du monde après que son action aura réussi, vaudra mieux que son état actuel. Son action suppose une évaluation des deux états, bref, des jugements de valeur. L'école de Vienne ne s'est jamais occupée des jugements de valeur. Il se pourrait que cette omission rende sa philosophie incomplète.

L'abstention des « empiristes logiques » devant ce problème était d'ailleurs naturelle. Ils s'occupaient de science et la science ne fournit jamais d'évaluation. Elle nous dit ce que les choses sont. Elle peut prévoir de manière assez générale, ce qu'elles deviendront. Jamais elle ne nous dit si elles sont bonnes ou mauvaises. Le savant ne pose pas, ne peut poser ce problème. Mais l'homme se le pose avec une anxiété lancinante, et il a besoin de le résoudre. La philosophie refusera-t-elle de le guider dans cette activité de nature nouvelle, où la science ne peut rien pour lui?

Deux réponses ont été faites, à ma connaissance, par les néo-positivistes pour justifier cette carence. La première consiste à nier l'existence des jugements de valeur. Elle soutient qu'ils se réduisent à des jugements de vérité, qui peuvent se traiter, comme tous les jugements de vérité, par les méthodes

de la science. La réduction du jugement de valeur au jugement de vérité s'opère par la notion de critérium. Tant que le jugement de valeur reste dans la forme que lui donne le commun des hommes, il reste de la pensée confuse et vague. Il y a un moyen de le clarifier et de le rendre un objet digne de notre méditation. Il suffit de préciser les conditions auxquelles la valeur s'applique à un objet, en d'autres termes d'élaborer un critérium. Aussitôt le critérium défini, le jugement de valeur aura disparu et sera remplacé par une série de jugements de vérité.

On dit que cette voiture automobile est excellente. Demandons à l'auteur du jugement de préciser sa pensée. Il finira, sous nos questions, par nous dire par exemple qu'une excellente voiture est celle qui tient bien la route et qui est capable de faire des moyennes peu inférieures à la vitesse à laquelle on roule habituellement. Nous aurons peut-être encore quelques éclaircissements à réclamer de lui — il y a cet inquiétant « tient bien la route » — mais, au bout de peu de temps, nous serons en possession d'une liste de caractéristiques. Nous pourrons vérifier expérimentalement si une voiture quelconque possède chacune de ces caractéristiques. Le critérium est établi. Le jugement de valeur a disparu.

Est-ce bien vrai? Le critérium peut bien se présenter sous la forme d'une définition — celle de l'excellence d'une voiture — il est à craindre que cette définition ne soit pas de la même nature que les paisibles définitions nominales. Ces dernières ne sont que des conventions de langage. On peut les construire absolument comme on veut. Certes, certaines sont plus commodes, mais un peu de commodité en plus ou en moins ne provoque pas d'hostilité bien farouche. Il faudrait un bien mauvais caractère pour provoquer une querelle à propos de définitions nominales. Mais les définitions de critérium, hélas! sont loin d'être dans ce cas. Pensons à la définition de la « vraie démocratie », et nous verrons les hommes agités de passions violentes à la pensée d'abandonner tel critérium, d'en accepter tel autre. Pour un peu, ils s'égorgeraient. Il doit y avoir une raison à ces réactions ardentes.

On la découvrira dès qu'on se sera rendu compte que, tandis que la définition nominale se borne à définir le mot « démocratie », le critérium définit la « vraie démocratie », c'est-à-dire la seule forme de démocratie estimable, auprès de

laquelle les autres conceptions ne sont que des altérations frelatées. Le critérium, sous une forme analytique, exprime un véritable jugement de valeur, qui a une nature profondément différente du jugement de vérité. Le choix même des caractéristiques dont la réunion constitue le critérium, signifie que ces caractéristiques *valent*. On est bien loin des méthodes scientifiques qui établissent l'accord entre les esprits par des voies éprouvées et sûres. Si deux hommes diffèrent sur la définition du critérium, leur différend reste irréductible. Car leur choix du critérium oriente leur vie d'une manière à laquelle ils sont profondément attachés. Ceci permet de conclure que le jugement de valeur peut être clarifié sans doute, quand on réussit à le transformer en critérium, mais qu'il ne perd en rien, par cette opération, sa véritable nature qui le sépare et l'oppose au jugement scientifique.

L'autre réponse que les positivistes ont souvent faite à la même objection, c'est que les jugements de valeur comme les actions humaines qui y obéissent sont « de simples manifestations vitales ». Nous aurions tort de nous scandaliser de cette réponse. Elle ne signifie pas, en effet, que les néo-positivistes refusent d'attacher aucune importance aux jugements de valeur. Une attitude semblable serait purement paradoxale, puisque les jugements de valeur sont la partie de sa personnalité à laquelle l'homme — et l'homme néo-positiviste comme les autres — est le plus profondément attaché. Ce que les néo-positivistes veulent dire, c'est que ces jugements ne sont pas d'authentiques connaissances, mais bien des moments réels de la vie humaine, des parties intégrantes de l'action. Or, les actions ne regardent pas le philosophe, dont la tâche exclusive est de clarifier ou de purifier la science. Les actions, appartenant à l'univers réel, regardent le savant. Elles concernent le psychologue et le sociologue. Quand ces savants auront obtenu des résultats positifs sur l'action humaine, le philosophe soumettra le langage qu'il auront employé à la même critique attentive qu'il a tournée déjà vers les sciences de la nature. Mais il ne lui appartient pas de traiter de la réalité, avant qu'elle n'ait subi l'élaboration scientifique.

Cette position est d'ailleurs parfaitement compatible avec la méthode du critérium qui vient d'être discutée, mais elle est plus ample et plus profonde. Elle fait mieux comprendre l'attitude de l'école de Vienne vis à vis de ce qu'on appelle,

d'une expression d'ailleurs malheureuse, les sciences normatives. Elle mérite un examen attentif.

Est-il bien vrai que le psychologue ou le sociologue soient les seuls à pouvoir nous apporter sur la conduite des éclaircissements qui vaillent d'être retenus. Ils procèdent par les méthodes usuelles de la science. Nous n'insisterons pas sur la relative pauvreté des résultats qu'ils ont jusqu'à présent obtenus dans ce domaine. Comparées à l'ensemble majestueux des lois physiques ou biologiques, les lois psychologiques ou les lois sociologiques font vraiment pauvre figure. Mais on les supposera fécondes par provision. Il reste qu'elles n'atteindront jamais que des généralisations inductives, comme le font leurs aînées, les sciences de la nature. Ne pouvons-nous obtenir, par d'autres voies, des résultats plus immédiats et peut-être plus précieux? La chose vaut d'être examinée avec soin et ne pourra devenir claire qu'au contact d'un exemple.

III

Les hommes poursuivent-ils tous une fin unique? On entend bien qu'il s'agit d'une fin ultime, et non de la poursuite d'objets qui se révèlent à l'analyse, n'être poursuivis qu'en vue d'autre chose. Ou bien les hommes poursuivent-ils des fins radicalement irréductibles? On voudra bien comprendre par fin, un objet concret, réel ou idéal, auquel s'attache une valeur — ce qui exclut, d'une part, les formes vides, comme le bien général, d'autre part, les états subjectifs, comme le bonheur ou le plaisir.

Ce problème est un des problèmes centraux de la morale. Mais, de plus, il a une réaction immédiate sur la question débattue. Si la fin ultime des efforts des hommes est unique, tous les désaccords qui peuvent surgir entre eux sur des questions de conduite deviennent des problèmes de choix des moyens les meilleurs pour atteindre la fin commune. L'adaptation de moyens à une fin constitue une technique, dans un sens très large du mot technique. Même dans ce sens large, une technique est toujours du savoir appliqué. Et si les hommes poursuivent une fin commune, la science suffisamment consultée pourrait indubitablement les mettre d'accord.

Nous serions ainsi bien près d'une solution favorable aux néo-positivistes. Il n'y aurait plus lieu de considérer les juge-

ments de valeur comme des termes de pensée originaux. Il resterait vrai que la science ne pose pas de jugement de valeur et vrai aussi que la morale ne peut reposer que sur des jugements de valeur. Mais la base de la morale serait l'unique jugement de valeur, commun à tous, et qui ne serait donc jamais mis en doute par personne. Les moyens de l'incarner dans nos actes, ce serait à la science de nous les donner.

IV

Si ce problème de la fin unique a aussi bien une importance générale qu'une valeur décisive pour l'analyse ici entreprise, il faut mettre le plus grand soin à le résoudre. A première vue, il paraît une question purement empirique. Il semble qu'on doive l'aborder en examinant historiquement la conduite des hommes, et discerner s'ils sont ou non guidés par une fin commune.

A un examen plus précis, des difficultés imprévues vont barrer le chemin à cette méthode. Nous venons de traverser une cruelle période, où l'humanité ramassée en plusieurs camps, par des fois diverses, a cessé de croire qu'il était possible de trouver un terrain d'entente. Les fois diverses étaient trop contradictoires pour qu'une discussion aboutisse. Il n'y avait plus d'autre moyen que les armes, et la guerre éclata. Cette première expérience nous montre donc les hommes poursuivant des fins incompatibles.

Si, pour simplifier les choses, on se borne à deux de ces fois, la foi démocratique et la foi nationale-socialiste, elles semblent bien s'opposer de tous points. Tandis que l'une affirmait — chacun sait qu'aujourd'hui un national-socialiste est un être introuvable — une foncière inégalité des hommes, basée sur le sang qui coulait dans leurs veines, les démocrates affirment que toutes les créatures humaines sont égales en dignité. Tandis que la démocratie dérivait de la dignité de l'individu la nécessité de lui donner une liberté de plus en plus complète, le national-socialisme asservissait l'individu, même et surtout quand il appartenait au *Herrenvolk*, à la toute-puissance de l'Etat. Il n'y a guère eu dans l'histoire d'antithèse plus complètement établie. Comme des gens de bonne foi ont poursuivi dans les deux camps la réalisation des

deux idéaux, la conclusion qui s'offrirait est que les hommes peuvent poursuivre des fins diamétralement opposées.

Ce serait compter sans la souplesse de la pensée humaine. Ressuscitons un théoricien national-socialiste, et il nous tiendra ce langage, partiellement historique, partiellement supposé : « Vous me parlez de dignité humaine : c'était le fond même de notre doctrine. N'oubliez pas que le racisme n'a jamais été pour nous une vérité historique, mais un impératif moral. Nous savions bien qu'il n'existe pas de races. Nous voulions en créer une. Une race, c'est un groupe d'hommes qui se soumet à des devoirs plus durs, plus exigeants que le commun des mortels. Et c'est par cette dignité supérieure que ce groupe serait devenu le *Herrenvolk*. Vous placez la dignité humaine dans la liberté de l'individu. Quelle erreur est la vôtre! Donnez à l'individu une pleine liberté, et il va se vautrer aussitôt dans la platitude de petits égoïsmes sans grandeur. Vous n'aurez même pas assuré son bonheur : la poursuite de ses avantages ne donne pas la paix de l'âme. L'homme ne peut trouver dignité et paix intérieure que s'il travaille à quelque chose de plus grand que lui, s'il s'oublie dans ce tout. En forçant rudement ceux que nous voulions former à se dévouer gratuitement à leur Etat, nous donnions à chacun ce dont tout homme a besoin, une dignité nouvelle. »

Si nous acceptons ces paroles comme un discours de bonne foi, nous allons être rejetés dans l'incertitude. Les deux idéaux, qui paraissent si radicalement opposés, poursuivent à présent une fin unique : la dignité de l'homme. Ils ne diffèrent que par les moyens employés. L'idéal démocratique émancipe l'individu, l'idéal national-socialiste lui commande de s'absorber dans l'Etat. Je repousserais sans doute ce dernier moyen d'assurer la dignité humaine, et cela me donne assurément le droit d'affirmer que mon national-socialiste se trompe, mais pas celui de nier qu'il poursuivait le même idéal de dignité humaine.

Ce que nous venons de faire pour les idéaux politiques qui se sont fait la guerre, nous pourrions le faire pour tous les idéaux en conflit. On en sera convaincu si on considère que le cas que nous venons d'examiner est incontestablement un des plus irréconciliables. Devrons-nous donc admettre que les hommes, en dépit de leurs divergences les plus violentes, poursuivent toujours une fin commune?

Ce serait aller trop vite en besogne. Il y a encore une question à résoudre avant de terminer l'examen. Elle demande à savoir si l'abstraction, en l'occurence, la dignité humaine, que nous sommes forcés de placer au terme des deux idéaux, est encore une fin réelle et non pas une forme vide. Le problème est délicat, car une fin morale est toujours une abstraction. Si l'application de cette abstraction doit finalement se résoudre à un ordre concret, qui détermine l'attitude du sujet vis-à-vis d'êtres singuliers, ces êtres singuliers ne font pourtant jamais partie intégrante de la règle morale, qui reste toujours abstraite. Mais, encore une fois, certaines abstractions sont les vrais opérateurs de la vie morale, d'autres restent des formes vides. « Tu ne voleras point » demeure évidemment une forme vide, tant qu'on n'a pas défini avec précision le sens du mot voler. Cette définition sera abstraite, car elle sera l'énumération de toutes les manières illégitimes de s'approprier le bien d'autrui. Tant qu'elle ne sera pas faite de manière exhaustive, nous serons dans l'incertitude vis-à-vis de certaines actions. Or, il n'y aura jamais de certitude d'avoir atteint tout le contenu du précepte « tu ne voleras point » dicté par notre conscience. Aucune preuve positive n'est à notre disposition qui nous permettrait de déclarer à coup sûr que nous nous trouvons ou bien devant une règle authentique ou devant une abstraction qui exige plus de détermination.

Cette incertitude n'a aucune importance pratique, car l'aiguillon de la conscience est toujours là dans l'expérience pour nous ramener dans le droit chemin si l'abstraction trop lâche nous conduisait à des actions que nous réprouverions. Mais elle a une importance théorique énorme. Elle montre, en effet, l'impossibilité de résoudre le problème de la fin unique. Il nous sera également impossible de prouver que les hommes poursuivent des fins incompatibles, car il sera toujours possible de subsumer ces fins sous une appellation commune. Et également impossible d'admettre cette appellation commune au rang des règles morales authentiques. La raison se dérobe.

V

Pourtant, le problème est urgent. Il faut le trancher. L'homme n'a pas le temps d'attendre que la pensée spéculative ait définitivement clarifié le débat, si elle en est capable un

jour. Il faut vivre, et tout de suite. Et d'importantes décisions dépendent du fait d'admettre ou que la fin poursuivie par les hommes est unique ou que ces fins diffèrent de sujet à sujet.

Ici se présente une autre méthode à laquelle le moraliste doit recourir en mainte occasion : celle de l'évaluation. On renonce aux voies de la raison théorique pour consulter directement la conscience. Mais il faut lui clarifier le problème.

Une première conclusion, obtenue plus haut, en fournit le moyen. S'il y a une fin unique, les seules incertitudes porteront sur les moyens de l'atteindre. Et l'étude des moyens en vue d'une fin ressort de la science. Il y aurait ainsi une vérité objective en morale, comme il y en a une en science.

Au contraire, si les fins des hommes varient de sujet à sujet, la conscience individuelle est l'unique tribunal qui ait le droit de décision. La conscience morale est ainsi proclamée autonome, échappant à toute autorité quelle qu'elle soit.

Il faut évaluer ces deux attitudes. La première introduit dans la conduite, les habitudes que nous avons en science. Or, elles sont étrangement intolérantes. Celui qui n'admettrait pas, après avoir pris connaissance de ses preuves, une vérité universellement reçue — on ne parle naturellement pas ici d'une vérité encore débattue sur le front du savoir — serait immanquablement jugé anormal. Il court le plus grand risque d'être isolé et retranché de la communauté. Ajoutons tout de suite que cette intolérance radicale n'introduit aucune violence parmi les hommes. Car la vérité scientifique se présente toujours avec ses preuves. Elle pénètre ainsi les esprits par sa propre force, sans qu'aucune coercition ne soit nécessaire.

Si ces méthodes sont transportées dans le champ des valeurs, elles se révéleront désastreuses. Ici, de preuves méthodiques, il n'est pas question. Il n'y a aucune méthode efficace pour mettre d'accord deux hommes dont l'un penserait que la propriété privée des moyens de production est un vol et l'autre croirait que cette même propriété privée est l'indispensable base d'une vie économique progressive. L'absence de preuves convainquantes laissera place à la violence. Ce qui signifiera que ceux qui auront la force contraindront les faibles à penser ce qu'eux croient être la vérité morale — c'est-à-dire leurs propres croyances morales. Et comme les faibles, par hypothèse, ne partagent pas spontanément ces croyances, la cité sera en proie à toutes les oppressions. De nos jours, ceux

qui ont la force, sont ceux qui se sont rendus maîtres de l'Etat. Croire que les hommes poursuivent une fin unique, c'est accepter de remettre aux puissants, le plus souvent à l'Etat, le droit de décider souverainement du bien et du mal.

Si l'autonomie de la conscience morale a rencontré chez certains quelque résistance, c'est parce que, par un jugement superficiel, on identifie le jugement de la conscience, à l'arbitraire du caprice, et qu'on tire cette conclusion surprenante que les hommes, laissés à eux-mêmes, sombreraient aussitôt dans l'immoralité. Il faut, pour penser ainsi, n'avoir pas perçu que, quels que soient les systèmes d'autorité dont on la ligote, la conscience morale demeure souveraine. Le bandit italien qui priait dévotement la Vierge de lui amener un riche voyageur à dépouiller, en est un excellent témoignage. Il acceptait la discipline de l'Eglise, mais sa conscience dévoyée empêchait cette discipline de se montrer efficace.

Reconnaître que l'homme poursuit des fins multiples, c'est proclamer que l'homme ne ressortit que de sa conscience. C'est proclamer aussi la valeur de la personne humaine, qui a droit au respect intégral de toute personne humaine.

Entre la cité où l'Etat dicte la moralité et l'Etat qui sera aménagé sur la base du respect des libres décisions de chacun, le choix doit se faire. Mais une fois le choix fait, on aura tranché du même coup le problème de l'irréductibilité des jugements de valeur aux jugements de vérité.

On aura compris ainsi pourquoi l'œuvre de l'empirisme logique nous paraît étrangement incomplète. Féconde dans le domaine de l'épistémologie, il lui manque un complément nécessaire. Ce qu'il y a d'ennuyeux, c'est que pour des esprits toujours plus nombreux, la part qui manque au système des néo-positivistes, est celle où se trouve le centre de gravité d'une philosophie.

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NOTES ET DISCUSSIONS

M. R. Hubert
et la construction du réel psychologique et pédagogique

L'œuvre de M. R. Hubert, recteur de l'Université de Strasbourg, dans le domaine de la psychologie et de la pédagogie, constitue un « cas » unique, et par là même remarquable, d'application de la méthode philosophique à la compréhension de la science. Il n'est pas douteux que cette œuvre intéresse le public des psychologues et des pédagogues, et qu'elle leur fournisse notamment un précieux outil de travail. Même en Belgique, où l'évolution de la pédagogie et de la psychologie appliquée a pris une orientation d'apparence rigoureusement positive (et positive dans le sens combien radical), les livres de M. R. Hubert sont de plus en plus cités et consultés par les étudiants, ceux-là mêmes qui seront les chercheurs de demain. Il est donc nécessaire d'être attentif à l'inflexion nouvelle dans laquelle les traités de M. R. Hubert engagent les spécialistes de l'enfance, de l'adolescence et de la pédagogie. Certes, ceux-ci ne s'inquiètent point du vaste dessein philosophique dont les livres, dont on parle dans cette brève étude, ne forment que des épisodes. Le plus souvent fermés à la pensée désintéressée — moins par vocation que par parti pris et manque d'information —, ils démontrent par leur curiosité que les ouvrages des philosophes possèdent une signification générale et spéciale multivalente.

Disciple d'Hamelin, dont P. Decoster nous fit connaître naguère l'ardeur et l'ingéniosité sans pareilles dans la construction du réel, M. R. Hubert est fidèle comme son maître à une inspiration idéaliste foncière. Mais sa méditation a largement dépassé l'œuvre de son maître, mort prématurément. L'ample dessein de la déduction embrasse ici autre chose encore que la déduction de l'esprit et de la réalité : elle s'étend aux valeurs morales et esthétiques. Ce n'est pas le lieu d'exposer cette grande conception. Ce qui en est dit vaut seulement pour situer une réflexion d'apparence plus modeste puisqu'elle ne pénètre que la psychologie et la pédagogie. Mais notre aveu signifie que l'œuvre récente s'intègre dans une entreprise plus vaste et explique sans doute aussi le plan d'un des ouvrages dont il va être question, à savoir le *Traité de Pédagogie générale.*

Le *Traité de Pédagogie générale* (Presses universitaires de France, collection Logos, 1946), après avoir exposé des définitions préliminaires dans l'introduction, consacre une première partie à la formulation des antinomies pédagogiques (l'éducation relativement à la nature, relativement à la Société et relativement à l'individu). Nous avons nous-même,

dans des articles récents, énuméré des antinomies de même ordre [1], pour ne pas admettre avec l'auteur que ces antinomies signalent les dimensions du problème de l'éducation. Si notre point de départ était différent (la définition de la pédagogie en tant que discipline de recherche et d'explication), il n'en reste pas moins que c'est — en ces matières — les difficultés qui sont révélatrices et non la doctrine unilatérale que l'on adopte souvent pour les surmonter.

Dans une deuxième partie, l'auteur s'intéresse aux bases expérimentales de l'éducation (les données biologiques, sociologiques et psychologiques qui répondent en quelque sorte aux trois antinomies préalablement énumérées). Mais c'est dans la troisième partie que se révèle précisément la vaste entreprise de M. Hubert et, comme nous le disions plus haut, l'intégration de la pédagogie dans son architecture. En effet, l'auteur y recherche les principes philosophiques d'une doctrine de l'éducation. Ces principes sont ceux d'un idéalisme spiritualiste (Chapitre I : *Esquisse d'une philosophie de l'esprit comme fondement de la pédagogie*. On consultera surtout le paragraphe 4 : *Genèse et structure de l'expérience*, et le paragraphe 7 : *De la communion spirituelle comme moyen de l'éducation*. — Chapitre II : *Conscience et Caractère; Psychologie critique*. — Chapitre III : *Conscience et Société; Sociologie critique*).

L'ouvrage comporte encore trois autres parties qui entraînent le lecteur dans une vaste synthèse de la pédagogie contemporaine. Aucun élément n'est oublié dans cette somme qui fournit au chercheur le résumé des publications les plus récentes, une bibliographie fouillée et une compréhension parfaite des problèmes.

L'Histoire de la Pédagogie (Presses universitaires de France, 1949) constitue une remarquable contribution aux disciplines pédagogiques. Partant de l'hypothèse que la pédagogie concrète — c'est-à-dire l'œuvre d'éducation actuelle — ne peut se constituer que sur des bases solides, M. R. Hubert édifie en quelque sorte, dans cet ouvrage, une de celles-ci. On sait que sous l'influence de Compayre l'histoire de la pédagogie avait en France pris un certain essor. Mais cet essor s'était arrêté, et l'œuvre de Compayre — aujourd'hui vieillie — n'avait guère suscité des recherches de vaste envergure. Il faut ajouter que sous l'inspiration de Durkheim l'explication des faits du passé pédagogique se trouvait enrichie d'une causalité nouvelle. Mais ce ne sont point seulement les doctrines du passé qui doivent s'expliquer par leur origine et par la description du milieu qui les a vues naître. L'histoire des doctrines pédagogiques ne constitue manifestement qu'un aspect du passé : il est des pensées généreuses qui ne sont jamais traduites en réalisations. Il est presque une erreur de considérer ce passé comme tributaire seulement de systèmes d'idées, si critiques, si nobles et si réalistes fussent-ils. La causalité sociale des phénomènes pédagogiques s'incrit tout entière dans les institutions. C'est assurément un grand mérite de ce livre de M. R. Hubert d'avoir, à côté de l'histoire des doctrines, énoncé une histoire des institutions. Nous sommes persuadés que cette méthode sera suivie ailleurs qu'en France. En ce domaine M. R. Hubert apparaîtra comme un novateur. L'auteur, d'ailleurs, élève le débat lorsqu'il déclare : « Une histoire de la pédagogie est, en effet, à sa façon, une histoire de l'esprit humain, puisqu'elle est la description des formations

[1] Cf. S. De Coster, *Les conditions limites de l'action éducative* (quatre articles dans la revue *Prométhée*, 1948-1949).

successives qu'il a reçues comme de celles qu'aux différentes époques de grands penseurs ont souhaité qu'il reçût. » Et d'ajouter : « L'histoire de la pédagogie est l'indispensable complément de l'histoire de la philosophie. » (Préface).

L'ouvrage se décompose en deux parties. La première est consacrée à l'exposé des faits pédagogiques (Chapitre I : *Les types d'éducation, des primitifs à la fin du moyen âge.* — Chapitre II : *L'époque moderne jusqu'à la fin du* XIXe *siècle.* — Chapitre III : *Les institutions pédagogiques à l'époque contemporaine*). Dans cette partie, les faits sont exposés sans recours aux doctrines : ces trois chapitres racontent, en une fresque brossée avec décision, les efforts des peuples de l'Europe et d'ailleurs apportés à la construction de leurs institutions d'enseignement. On peut féliciter l'auteur d'avoir mis l'époque contemporaine dans le prolongement des époques précédentes. Les problèmes actuels sont les problèmes du passé mais amplifiés et rendus plus imbriqués, plus considérables aussi par l'accession du peuple à la culture. Dans de nombreux pays, les conceptions actuelles sont à l'image de la tradition; ou du moins empruntent à celle-ci leurs caractères profonds et durables. Peut-être le paragraphe consacré aux primitifs eût-il pu mieux révéler à la fois les différences et les similitudes de leur éducation et de celle des cités grecques primitives. On voit bien chez les primitifs, à mesure qu'ils évoluent, l'apparition d'un système d'éducation *institutionnelle.* Le premier pas dans cette élaboration d'intitutions réside dans la séparation des âges, celle-là même qui à Sparte constituait la base de l'organisation sociale. On ne voit point non plus, comme on pouvait l'attendre après la remarquable *Histoire de l'Education dans l'Antiquité* de M. Marrous, apparaître à un moment donné le classicisme grec avec sa signification culturelle, son mode d'instruction et d'éducation, qui allait servir d'exemple à la pédagogie de l'enseignement secondaire jusqu'à l'époque contemporaine [1]. Par contre, dans l'explication des doctrines, le philosophe qu'est M. Hubert se meut à l'aise et offre au lecteur la surprise de clartés nouvelles et d'aperçus fins et pénétrants. La deuxième partie est, en effet, consacrée à cet exposé. Ses quatre chapitres répondent aux trois chapitres de la première partie (Chapitre I : *Doctrines antiques.* — Chapitres II et III : *Doctrines modernes.* — Chapitre IV : *Tendances de la pédagogie contemporaine*).

Comme le *Traité de Pédagogie générale*, l'ouvrage constitue un outil précieux à la recherche grâce au souci du détail, à l'ampleur du plan, à la bibliographie.

Enfin, sans que cette vérité soit exprimée, il apparaît de plus en plus à la lecture des deux livres de M. Hubert, déjà signalés dans cette étude, que la pédagogie — dans tous ses aspects — constitue à l'heure actuelle un groupe de disciplines mineures où des méthodes synthétiques doivent être utilisées. Nous nous sommes bien souvent expliqué sur cette nécessité [2] pour ne point considérer que l'œuvre de M. R. Hubert forme une démonstration évidente de cette vérité.

D'une tout autre inspiration sont les deux récents ouvrages que M. R. Hubert a intitulés *La Croissance mentale, Etude de Psychogénétique* (Paris, Presses universitaires de France, 1949). Le premier volume est tout entier consacré à l'enfance, le second à l'adolescence.

[1] Par exemple, le nom d'Isocrate n'est pas signalé!

[2] Cf. S. De Coster, *La Pédagogie, discipline de la complémentarité* (*Revue des Sciences pédagogiques*, tome XI. 1949, n° 451).

Les deux volumes se caractérisent par la même rigueur et par le même luxe de renseignements.

L'auteur utilise deux grandes hypothèses, à savoir : *a*) l'idée que la psychogénèse se développe selon un rythme d'oscillations, la croissance ne se faisant pas régulièrement; *b*) l'idée que ce développement est commandé par sa fin, l'arrivée de l'individu à un stade de conscience intellectuelle. La première de ces idées nous l'avons expliquée nous-même dans nos travaux théoriques et pratiques : elle nous paraît le squelette de toute conception valable sur la psychogénèse[1].

On comprend dès lors que le titre de l'ouvrage soit la croissance mentale, les fonctions intellectuelles étant considérées comme primant relativement aux fonctions affectives et actives — mais à l'aboutissement du processus et non à son origine.

Il est malaisé de résumer un travail de cette nature qui vient à son heure pour donner au public des chercheurs une synthèse — mais non une synthèse dispersive et noyée dans les détails — une synthèse commandée par des idées directrices. Les bibliographies si étendues, la division des périodes, le style aisé font de cette synthèse un outil de travail précieux entre tous.

Si l'on veut maintenant, au terme de ce triple exposé, observer les similitudes de conception foncière, on en arrivera rapidement à la conclusion que les trois ouvrages analysés développent une même idée. Ils constituent, en vérité, une défense de la philosophie de l'Esprit. Tantôt, M. Hubert montre, par la psychogénèse, par quels stades l'esprit humain passe pour aboutir à participer à cet Esprit, tantôt il enseigne que l'éducation doit aider cette évolution par des techniques appropriées, tantôt encore il relate les épisodes de l'effort humain dans l'œuvre éducative qui, peu à peu, et au fil des siècles, doit aboutir à consolider ce que la doctrine a prévu.

Il ne s'agit donc plus, pour cet idéaliste, de formuler les catégories qui expliquent l'odyssée de l'esprit à la conquête du réel; il ne s'agit plus de faire la critique de l'entendement au nom de quelque double expérience où, du choc de la sensibilité, l'esprit construit le réel et le projette à nouveau dans cette même sensibilité. M. R. Hubert dépasse largement l'idéalisme kantien et hamélien. A ceux qui voudraient connaître ce progrès, il n'est que de renvoyer aux pages du *Traité de Pédagogie générale* où la justification de ce progrès est proposée (notamment p. 202). Nous ne nous étions prescrit dans cette *note* que de prouver comment se construit le réel psychologique et pédagogique dans les œuvres récentes de M. R. Hubert. Nous ajouterons, à partir de la conscience appréhendant l'Esprit pour la communion des hommes.

Sylvain De Coster (*Bruxelles*).

Bibliographie

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3. René Hubert, *La croissance mentale. Etude de Psychogénétique*. Vol. I : *L'Enfance;* vol. II : *L'Adolescence*, Paris, Presses universitaires de France, 1949, 638 pages.

[1] Cf. S. De Coster, *Une Expérience d'individualisation et de coordination des études*, Bruxelles, Ligue de l'Enseignement, document n° 131, 1949, p 5.

ANALYSES ET COMPTES RENDUS

Thadée CZEŻOWSKI, *Logika*, Editions scolaires de l'Etat, Varsovie, 1949, 273 pages (600 zloty).

Grâce à la décision, prise en 1938, de la commission des manuels universitaires, M. Czeżowski a pu faire paraître son cours de logique destiné aux étudiants en philosophie. Ce cours, rédigé par un philosophe — et non par un mathématicien — se limite à l'étude de la logique formelle et évite soigneusement toute incursion dans le domaine de la psychologie ou de l'épistémologie. Son mérite principal est de fournir l'essentiel des résultats de la logique moderne qui ont quelque intérêt pour les philosophes, en montrant d'ailleurs en quoi ils reprennent, et en quoi ils modifient, la logique aristotélicienne. Le manuel comporte sept parties qui exposent les fondements sémantiques de la logique, la théorie des propositions, la théorie des quantificateurs, celle des prédicats et des relations, une théorie des probabilités et une esquisse du développement historique de la logique; le volume est pourvu d'une bibliographie, suffisante en ce qui concerne les auteurs polonais, mais qui ne tient aucun compte d'ouvrages étrangers parus après 1936, ce qui s'explique d'ailleurs par les événements de guerre. Ce qui caractérise ce manuel, qui par sa clarté, sa simplicité et sa rigueur constitue un modèle du genre, c'est la place importante, méritée d'ailleurs, réservée à l'école polonaise de logique, et en particulier aux travaux de Lukasiewicz et de Lesniewski. C'est sur les vues de ce dernier que sont basés toute la sémantique de l'auteur, son exposé concernant les catégories sémantiques et la manière dont leur distinction permet d'éviter les paradoxes de la logique.

Ch. P.

H. REGNÉLL, *Symbolization and Fictional Reference, a Study in Epistemology*, Lund, C. W. K. Gleerup, s. d. [1949], 1 vol. 24 × 16.

L'ouvrage que nous présentons ici se place, à notre avis, très manifestement dans le courant des études sémantiques nées sous l'influence, d'une part, de la tendance positive vers laquelle a évolué l'école d'Uppsala (Hagerstroem, Phalén) et, d'autre part, des idées apportées à Lund par Ernst Cassirer lors du bref séjour qu'il y fit (on se rappellera, de ce dernier, la *Philosophie der Symbolischen Formen*).

Regnéll divise son ouvrage en deux parties, dont la seconde est une application à l'arithmétique, à la géométrie, à la physique, à la religion et à la logique des notions fondamentales énoncées dans la première partie.

Il nous semble regrettable que cette application soit si pauvre : l'auteur n'y exploite pas ses conclusions au maximum dans ses tentatives vers la recherche du mode de désignation qui permet de passer du nombre à une quantité réelle, d'un point à un solide réel, d'une théorie physique à un ensemble de phénomènes observés.

Mais attachons-nous plutôt à l'analyse de la partie principale de *Symbolization and Fictionnal Reference.* On la trouvera dans la première partie, aux chapitres II et III.

Voyons, avant tout, comment Regnéll définit la notion de « reference I-S ».Ce symbole désigne, pour lui, la référence d'une unité d'ordre inférieur à une unité d'ordre supérieur. Une unité, dans le champ perceptif, est un des termes (au sens arithmétique du mot) d'une relation de différence. Toute unité suppose donc un rapport. L'auteur affirme aussi la réciproque : tout rapport suppose des unités. Or, dit-il, il y a des rapports d'ordre supérieur (rapports de rapports, etc.); donc, il y a aussi des unités d'ordre supérieur.

Et Regnéll nous donne comme exemple une tache de couleur changeante sur un fond blanc : les différentes couleurs par lesquelles passe la tache ne lui enlèvent pas son unité propre, et elle reste différente par rapport au fond; par cet exemple, on voit comment l'unité d'ordre supérieur (ici, le contraste entre la tache et le fond) domine l'unité inférieure (ici, les différentes couleurs par lesquelles passe la tache).

Cependant, note l'auteur, toute référence I-S n'est pas nécessairement une symbolisation. Il y a des références réelles et des références irréelles : une référence est irréelle si l'objet auquel on se réfère n'est qu'apparemment ce qu'on croit qu'il est, Un symbole devra toujours avoir une double référence I-S : une réelle et une irréelle; le signe « lion », par exemple, se réfère à tous ses contextes réels (linguistiques ou non), et aussi au lion en chair et en os dont il n'a jamais formé une partie et avec lequel il n'a jamais eu de rapport réel.

Mais — et ceci ajoute une troisième caractéristique — cette seconde référence irréelle, bien que la sachant telle, nous continuons à y croire. Ces expressions cachent, soit une illusion qui ne devient pas une hallucination, soit une convention librement acceptée, mais qu'on sait arbitraire. Un symbole se réfère donc fictivement à un objet. D'où on déduira que la relation de symbolisation, référence d'une unité d'ordre inférieur à une unité d'ordre supérieur, n'est pas une relation symétrique; et ceci démontre qu'il ne faut pas admettre la similitude comme base de cette relation de symbolisation.

En outre, on pourra déduire également du caractère fictif de la référence que la symbolisation n'est pas transitive.

Mais, remarque notre auteur, ces conditions ne sont pas suffisantes : des relations asymétriques et intransitives, temporelles (« après »), spatiales (« part de ») ou causales, ne constituent pas encore le symbolisme. Ce dernier suppose en effet la simultanéité et exige, soit une négation de distance, soit une différence de plan. On voit que tout dépend ici du sens des notions « apparence » et « fiction », et, corrélativement, de la notion de « croyance ».

L'auteur, en se basant sur cette définition fondamentale, rejette toute analyse purement behaviouriste, telle celle de Morris : il affirme en effet, d'abord, que l'analyse de ce dernier ne tient par compte de la référence fictive, et ensuite, que la référence fictive ne se décrit pas en général dans un langage non mentaliste. Regnéll analyse alors avec finesse quelques cas-limites : l'image mnémique, le tableau du peintre,

l'image réfléchie par le miroir, les images des images; et il y recherche — et retrouve — les caractéristiques essentielles de la notion de symbole, mais avec des complications dues en général à la multiplication des références irréelles.

Tentative ingénieuse, à n'en pas douter, que celle de Regnéll. Sa théorie se base, nous l'avons montré, sur les notions suivantes : « unités d'ordres différents », « apparence », « fiction », et sur les rapports qui existent entre ces notions. Mais il nous semble que l'auteur, cherchant à définir une notion complexe, celle de symbole, l'a définie par une notion plus complexe encore : celle de fiction. Cette seconde notion, outre sa difficulté, a un autre défaut bien plus grave : elle est elle-même incomplètement définie. Car, Regnéll nous l'a dit, pour comprendre un symbole, nous identifions ce symbole et la chose symbolisée, puis nous nions cette identification, pour l'accepter quand même à la fin, mais avec certaines réserves. Or, les deux premières attitudes (identification du symbole et du symbolisé et négation de cette identification) n'expriment en aucune manière ce qu'il y a de spécifique dans le fait que nous pouvons prendre conscience du signe en tant que tel. Et c'est seulement dans cette « acceptation sous certaines réserves », c'est-à-dire l'acceptation qui insiste sur le caractère fictif du rapport entre le signe et la chose signifiée, c'est seulement là que nous trouverons l'attitude spécifique devant le signe; mais c'est là aussi que nous regretterons que Regnéll ne définisse pas plus clairement ce qu'il entend par « fiction ».

A notre avis, si le rapport entre symboles est partiellement conventionnel, ce rapport lui-même (= fiction) n'est pas défini par cette simple indication de son origine.

Mais examinons maintenant le rapport entre les différentes notions :

En premier lieu, nous ne voyons pas, pour notre part, pour quelle raison le symbole et le symbolisé devraient être des unités d'ordre différent, une fois admise la seconde partie de la définition de Regnéll, à savoir ; le symbolisme comme référence fictive. Si, par exemple, le symbole est un objet ayant un certain rapport avec un autre objet, ne sont-ils pas, par définition, des unités du même ordre? Rien, dans la définition que donne Regnéll de la référence fictive, ne démontre qu'elle ne peut s'effectuer entre des unités du même ordre.

En second lieu, reprenons la définition que donne Regnéll de la réalité, et demandons-nous si elle est compatible avec sa définition du symbole par la fiction. Est réel, pour l'auteur, ce qui, dans un monde, observé par l'observateur le plus circonspect et le mieux équipé, obéit à un système de lois. Si donc la référence d'un signe, c'est-à-dire son appartenance réelle à son désigné, n'est que fictive, c'est qu'elle est irrégulière, variable, et n'obéit pas à un système de lois. Or Regnéll est sévère pour l'apparent : il élimine un psychisme, lieu des apparences et, soit relativise cette qualité d'« apparent », ce qui relativise aussi la qualité de « symbole », soit exclut tout à fait l'apparent de son système. On voit à quel point ceci rend précaire la place qu'occupe la fiction dans ce système.

Et nous arrivons ainsi à l'objection essentielle que nous voudrions opposer à Regnéll : sa définition même de l'« apparent » comme l'« irrégulier » empêche l'auteur de construire la symbolisation comme une référence fictive. En effet, selon ses propres définitions, toute référence qui serait fictive serait par le fait même irrégulière et devrait être exclue du système; ou, pour reprendre les paroles de l'auteur,

tout symbole serait symbole subjectif. Ceci impliquerait donc l'impossibilité de l'existence d'un langage comme système stable et comme ensemble de signes pourvus de signification constante.

Regnéll n'atteint donc pas, nous semble-t-il, le but qu'il se proposait, et nous sommes donc obligé de déplorer que ce livre, par ailleurs d'une belle tenue et qui témoigne de beaucoup de talent de la part de son auteur, n'apporte pas de conclusions satisfaisantes.

Mais Regnéll a le grand mérite de poser fort clairement un problème à notre connaissance peu traité jusqu'à ce jour : celui des rapports entre symbole, fiction et réalité.

On sent, dans cette étude, que l'auteur a été influencé de manière très féconde par ces admirables écoles analytiques, l'école anglaise et l'école scandinave, auxquelles la philosophie du xx[e] siècle doit sa principale orientation. Mais on ne peut s'empêcher, à la lecture de *Symbolization and Fictional Reference*, de penser à une autre analyse, un peu partiale sans doute, mais profonde : celle de ce pur behaviouriste qu'est Ch. Morris; et on ne peut s'empêcher de préférer la position de ce dernier à celle Regnéll.

L. Apostel.

Mario Pensa, *Das deutsche Denken*, Untersuchung über die Grundformen der deutschen Philosophie, traduit de l'italien par W. Meckauer, Eugen Rentsch Verlag, Erlenbach-Zürich, 1948, 411 pages.

L'ouvrage de M. Pensa présente une curieuse et originale tentative de préciser les formes caractéristiques de la pensée allemande telle que, depuis Albert le Grand juqu'à nos jours, elle s'est développée dans les écrits religieux, littéraires et surtout philosophiques.

Pour M. Pensa, la pensée allemande se distingue de la pensée classique, telle qu'elle se manifeste dans la tradition grecque et catholique, par sa problématique particulière. Alors que la pensée classique est d'inspiration logique, qu'elle est fondée sur l'opposition sujet-objet, réalité-connaissance, universel-particulier, et que les problèmes qu'elle s'efforce de résoudre peuvent être, à proprement parler (dit M. Pensa), appelés philosophiques, la pensée allemande serait caractérisée par le problème des rapports du moi — d'un moi transcendantal — à l'univers, par un irrationalisme accordant à la volonté le primat sur la connaissance, à l'éthique le primat sur la logique. Dans cet esprit, elle se développerait, à partir des mystiques allemands, en passant par Luther, jusqu'à Kant, qui dégage l'idée du moi transcendantal, et son couronnement aboutirait à Hegel, sur le plan théorique et systématique, et à Nietzsche, sur le plan moral.

Le point de vue de l'auteur l'amène à isoler la pensée allemande des influences qu'elle a pu subir et le conduit à s'attacher plus aux conclusions qu'à la structure systématique de tout développement philosophique. M. Pensa est ainsi entraîné à gauchir les conceptions d'un Liebniz ou d'un Kant, par exemple, en considérant leur rationalisme comme une sorte de vêtement, de présentation couvrant des thèses d'une inspiration toute différente. Néanmoins, quels que soient les inconvénients inhérents à une entreprise, dont la réalisation nécessite une optique particulière, nous pouvons féliciter l'auteur de l'avoir menée à bien en dégageant une ligne de développement très suggestive de cette pensée allemande qui a si profondément influencé la philo-

sophie européenne. Pourtant, malgré tout son intérêt pour cette pensée, l'auteur ne l'apprécie guère, et lui préfère nettement la pensée classique. Cette préférence, on ne pourrait mieux la faire connaître qu'en citant ces quelques lignes de la conclusion (p. 403), qui indiquent le ton général de l'ouvrage :

« Das deutsche Ich, das heisst das transzendentale Ich, stellt jenes Stadium des Bewusstseins dar, in welchem Geist und Natur noch in einer unbestimmten, ununterschiedenen Einheit verschmolzen sind. In ihm ist das Dualismus, die Zweiheit Subjekt und Objekt, Idee und Sache, noch nicht geboren, weil das Denken als logische Fähigkeit des Erkennes noch nicht erwacht ist. Dieses Stadium ist also vor-Philosophischer Art, es ist noch nicht Philosophie. Die deutsche Philosophie ist also eher eine Philo-Physik (ein Name, den Friedrich Schlegel für die Naturwissenschaft vorschlug) als eine φιλοσοφìα, das heisst als eine Wissenschaft des Erkennens und des Wissens. »

Ch. P.

Magdalena Aebli, *Kants Begündung der deutschen Philosophie*, Bâle, Recht und Gesellschaft A. G., 1947, 525 pages).

L'important ouvrage de Mlle M. Aebli constitue sans conteste la contribution la plus importante à l'exégèse kantienne depuis dix ans. Il est difficile dans l'espace trop réduit d'un compte rendu, d'analyser en détail la longue démonstration apportée par l'auteur à l'hypothèse directrice de son explication de la logique de la *Critique de la Raison pure*. Cette hypothèse apparaît déjà dans le titre du livre. Il y est question de la philosophie allemande et de la part que Kant a prise dans son élaboration. C'est donc d'une conception préliminaire de cette philosophie que procède l'analyse de Mlle Aebli. Et cette conception, on l'aperçoit immédiatement, considère Kant, en quelque sorte comme le fondateur de cette philosophie : en d'autres termes, les philosophes allemands ultérieurs adhéreraient à l'idéalisme critique par filiation directe ou par opposition. En ce sens une philosophie allemande existerait spécifiquement à côté d'une philosophie française et cette dernière devrait à Descartes et plus tardivement à Auguste Comte sa structure intime. Dans les formes récentes de l'existentialisme français, apparemment tout influencé par les systèmes allemands, cette structure se retrouverait encore. On peut, à l'aide de ces éclaircissements, énoncer la thèse fondamentale de Mlle Aebli. Il y aurait des modes nationaux de comprendre la réalité, d'appréhender ou de construire le monde objectif. Ces modes constitueraient des habitudes de pensée traditionnellement acceptées à partir d'un génie philosophique qui les aurait si l'on peut dire formulées.

Une telle conception heurte les historiens prudents. Elle n'est cependant ni plus ni moins subjective que celle consistant à relier les systèmes philosophiques aux doctrines politiques et qu'a illustrée un grand philosophe anglais. La démonstration de Mlle Aebli est conduite en profondeur, avec une remarquable lucidité. Sa connaissance de l'œuvre de Kant est minutieuse, mais toutes ses analyses sont clairement enchaînées. De sorte que son livre ne donne jamais cette impression de décousu et de poussière, caractéristiques de tant de commentaires de l'idéalisme critique. Par ailleurs, son ouvrage comporte un nombre élevé de tableaux synoptiques tracés avec soin, tableaux qui facilitent

considérablement la compréhension (pp. 9, 29, 39, 40, 41, etc... 496, 497). Indépendamment de la thèse de l'auteur et de sa démonstration, le livre de M[lle] Aebli forme à la fois un exposé, une analyse et un commentaire remarquable à tous égards de la logique kantienne. Toute la première partie de l'ouvrage est consacrée à l'énoncé de la notion kantienne de la logique transcendantale; la deuxième, au contraire, développe longuement la déduction de cette même logique. Si l'esthétique transcendantale constitue le chef-d'œuvre d'E. Kant, il est certain que l'établissement de la table des catégories, de celle des principes, etc., a demandé au philosophe de Koenigsberg un travail où s'affirme tout son génie. C'est dans cette partie de sa doctrine que l'exégèse cherche le plus volontiers les apories et les faiblesses; il n'en est aucune cependant qui soit aussi solidement charpentée, aussi difficilement accessible. Les obscurités de la déduction s'éclairent toutefois au commentaire de M[lle] Aebli. Il m'apparaît qu'aucun compliment meilleur ne pouvait lui être rendu, même si l'hypothèse directrice de son immense étude ne séduisait pas immédiatement le lecteur.

Sylvain De Coster.

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LA VIE PHILOSOPHIQUE

Décès

Nous avons appris avec infiniment de regret la mort à Ann Arbor, Michigan (U. S. A.), le 21 juin 1949, à l'âge de 64 ans, de l'esthéticien et philosophe américain Dewitt H. Parker qui fut un des premiers collaborateurs de la *Revue internationale de Philosophie* (n° 4, juillet 1939). Le professeur Dewitt H. Parker enseignait à l'Université de Michigan et était l'auteur notamment d'un ouvrage important : *Principles of Aesthetics* dont une seconde édition parut en 1947.

Cinquième Congrès des Sociétés de Philosophie de Langue française

Le cinquième Congrès des Sociétés de Philosophie de Langue française dont l'organisation incombe en 1950 à la Société de Philosophie de Bordeaux, présidée par le professeur René Lacroze, aura lieu à Bordeaux du 14 au 17 septembre 1950.

Le Comité d'organisation a choisi comme thème *Les Sciences et la Sagesse.*

Le Secrétaire général du Congrès est M. Ed. Morot-Sir, 20, Cours Pasteur, Bordeaux, France, auprès de qui peuvent être demandés tous renseignements.

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